DEXED

Philosophy of Science,

VOLUME 4, NUMBER 3

Published Quarterly by The Williams & Wilkins Company, Baltimore, Maryland

July, 1937

PHILOSOPHY OF SCIENCE

ORGAN OF THE PHILOSOPHY OF SCIENCE ASSOCIATION

The Editorial Board

WILLIAM MARIAS MALISOFF, *Editor*
New York, N. Y.

ERIC TEMPLE BELL
ALBERT E. BLUMBERG
RUDOLF CARNAP
MORRIS RAPHAEL COHEN
W. W. COOK
HERBERT FEIGL
K. S. LASHLEY
HENRY MARGENAU
H. J. MULLER
L. SUSAN STEBBING
DIRK J. STRUIK
ALEXANDER WEINSTEIN

The Advisory Board

EDGAR ALTENBURG
G. D. BIRKHOFF
MARCEL BOLL
P. W. BRIDGMAN
K. T. COMPTON
E. U. CONDON
W. J. CROZIER
JOHN DEWEY
F. G. DONNAN
L. C. DUNN
ALBERT EINSTEIN
R. A. FISHER
J. B. S. HALDANE
R. G. HARRISON
H. HENCKY
H. S. JENNINGS
K. KOFFKA
W. KÖHLER
K. LANDSTEINER
I. LANGMUIR
RALPH LILLIE
A. O. LOVEJOY
W. MAYER
W. P. MONTAGUE
T. H. MORGAN
CHAS. W. MORRIS
JOSEPH NEEDHAM
J. VON NEUMANN
F. S. C. NORTHROP
H. A. OVERSTREET
M. POLANYI
G. Y. RAINICH
N. RASHEVSKY
H. REICHENBACH
O. L. REISER
O. K. RICE
A. E. RUARK
GEORGE SARTON
C. E. SEASHORE
H. SHAPLEY
E. A. SINGER, JR.
HENRY B. SMITH
W. F. G. SWANN
R. C. TOLMAN
H. UREY
M. S. VALLARTA
PAUL WEISS
GERALD WENDT
HERMANN WEYL
A. N. WHITEHEAD
N. WIENER
E. WIGNER
SEWALL WRIGHT
F. ZWICKY

Entered as second class matter November 25, 1933, at the Post Office, at Baltimore, Maryland, under the Act of March 3, 1879.

Made in United States of America

Subscription price $5.00 per volume. Single numbers, $1.25

Philosophy of Science

VOL. 4 July, 1937 NO. 3

CONTENTS

Causality and Complementarity. Niels Bohr - - 289
The New Logic. Karl Menger - - - - - - - 299
Critical Points in Modern Physical Theory. Henry Margenau - - - - - - - - - - 337
Objectivity in the Social Sciences. Eleanor Bisbee - - - - - - - - - - - - - - 371
Discussion (Correspondence) - - - - - - - 383
Reviews and Notes - - - - - - - - - - - 385

Copyright 1937, The Williams & Wilkins Company

"Not a manufactured product, not a collection of theories—It Grew!"

PREPARATION OF SCIENTIFIC AND TECHNICAL PAPERS

By
SAM F. TRELEASE
and
EMMA SAREPTA YULE

A Manual for the use of those who write on scientific or technical subjects.

Third Edition—Revised and Enlarged

SUGGESTIONS ON SUBJECT MATTER AND ARRANGEMENT
THE CORRECT USE OF TENSES
PREPARATION OF TYPEWRITTEN COPY
CORRECTIONS AND ALTERATIONS ON MANUSCRIPT—WHAT TO AVOID
INDICATING TYPE TO BE USED
CAPITALIZATION AND ITALICIZATION
ABBREVIATIONS—CORRECT AND INCORRECT
TABLES, FOOTNOTES, ETC.
PREPARING TABLE OF CONTENTS AND INDEX
READING AND CORRECTING PROOF

Everything in short to answer the various maddening questions which arise whenever thoughts go on paper with a view of publication.

$1.50

THE WILLIAMS & WILKINS COMPANY
Publishers of Scientific Books and Periodicals
BALTIMORE, U. S. A.

Philosophy of Science

VOL. 4 July, 1937 NO. 3

Causality and Complementarity[1]

BY

NIELS BOHR

ON SEVERAL occasions[2] I have pointed out that the lesson taught us by recent developments in physics regarding the necessity of a constant extension of the frame of concepts appropriate for the classification of new experiences leads us to a general epistemological attitude which might help us to avoid apparent conceptual difficulties in other fields of science as well. Since, however, the opinion has been expressed from various sides that this attitude would appear to involve a mysticism incompatible with the true spirit of science, I am very glad to use the present opportunity of addressing this assembly of scientists working in quite different fields but united in their striving to find a common ground for our knowledge, to come back to this question, and above all to try to clear up the misunderstandings which have arisen.

Before entering into the problems to be discussed, I need recall only briefly how often the development of physics has taught us that a consistent application even of the most elementary concepts indispensable for the description of our daily experience, is based on assumptions initially unnoticed, the explicit consideration of which is, however,

[1] Address delivered before the Second International Congress for the Unity of Science, Copenhagen, June, 1936.

[2] "Atomic Theory and Description of Nature", four essays and an introductory survey; Cambridge, 1934; quoted in the text as A_I, A_{II}, A_{III}, A_{IV}, and A_E; further, "Light and Life" Nature 131, 421; 457, 1933; and "Can Quantum Mechanical Description of Physical Reality be Considered Complete?" Physical Review LXVIII, 696, 1935; quoted as B and C respectively.

essential if we wish to obtain a classification of more extended domains of experience as clear and as free from arbitrariness as possible. I also hardly need to emphasize how much this development has contributed to the general philosophical clarification of the presuppositions underlying human knowledge. Even though these acquisitions are in many respects of a lasting character, we have nevertheless received only recently an incisive admonition that the analysis of new experiences is liable to disclose again and again the unrecognized presuppositions for an unambiguous use of our most simple concepts, such as space-time description and causal connection.

It was in fact the clarification of the paradoxes connected with the finite velocity of propagation of light and the judgment of events by observers in relative motion which first disclosed the arbitrariness contained even in the concept of simultaneity, and thereby created a freer attitude toward the question of space-time coördination which finds expression in the theory of relativity. As is well known, this has made possible a unified formulation of the phenomena appearing in different frames of reference, and through this brought to light the fundamental equivalence of hitherto separate physical regularities. The recognition of the essential dependence of any physical phenomenon on the system of reference of the observer, which forms the characteristic feature of relativity theory, implies, however—as especially Einstein himself has emphasized—no abandonment whatever of the assumption underlying the ideal of causality, that the behavior of a physical object relative to a given system of coördinates is uniquely determined, quite independently of whether it is observed or not.

However, a still further revision of the problem of observation has since been made necessary by the discovery of the universal quantum of action, which has taught us that the whole mode of description of classical physics, including the theory of relativity, retains its adequacy only as long as all quantities of action entering into the description are large compared to Planck's quantum. When this is not the case, as in the region of atomic physics, there appear new uniformities which cannot be fitted into the frame of the ordinary causal description (A_I). This circumstance, at first sight paradoxical, finds its elucidation in the recognition that in this region it is no longer possible sharply to distinguish between the autonomous behavior of a physical object and its inevitable interaction with other bodies serving as measuring instruments, the direct consideration of which is excluded by the very nature of the concept of observation in itself (A_{II}).

Indeed this circumstance presents us with a situation concerning the analysis and synthesis of experience which is entirely new in physics and forces us to replace the ideal of causality by a more general viewpoint usually termed "complementarity." The apparently incompatible sorts of information about the behavior of the object under examination which we get by different experimental arrangements can clearly not be brought into connection with each other in the usual way, but may, as equally essential for an exhaustive account of all experience, be regarded as "complementary" to each other. In particular, the frustration of every attempt to analyse more closely the "individuality" of single atomic processes, symbolized by the quantum of action, by a subdivision of their course, is explained by the fact that each section in this course definable by a direct observation would demand a measuring arrangement which would be incompatible with the appearance of the uniformities considered. Notwithstanding all differences, a certain analogy between the postulate of relativity and the point of view of complementarity can be seen in this, that according to the former the laws which in consequence of the finite velocity of light appear in different forms depending on the choice of the frame of reference, are equivalent to one another, whereas, according to the latter the results obtained by different measuring arrangements apparently contradictory because of the finite size of the quantum of action, are logically compatible.

In order to give as clear an idea as possible of the new epistomological situation which we meet in atomic physics, we may briefly consider those measurements designed to obtain an account of the space-time course of some physical event. The account consists in the last analysis in the establishment of a series of unambiguous connections between the behavior of the object and the measuring rods and clocks which define the system of reference involved in the space-time description. It is thus only as long as we may completely ignore, in the description of all the important circumstances of the event, all interaction between the object and these measuring instruments, which unavoidably accompanies the establishment of any such connection, that we can speak of an autonomous space-time behavior of the object under observation, independent of the conditions of observation. In case, however—as in the region of quantum phenomena—this interaction plays an essential rôle for the appearance of the phenomena themselves, the situation is completely changed, and we are in particular forced to renounce the combination, characteristic of classical physical description, of the space-time coördination of the event with the general conservation

theorems of dynamics. For the use of rods and clocks to fix the system of reference makes it by definition impossible to take into account the energy of momentum which might be transferred to them in the course of the phenomenon. Conversely, those quantum laws whose formulation rests essentially on the application of the concept of energy or momentum can appear only under circumstances of investigation from which a detailed account of the space-time behavior of the object is excluded.

As is well known, a mode of description suitable to this situation has been found in the so-called quantum mechanics, in which sufficient freedom for the consistent coördination of the new regularities has been achieved by the substitution for the usual kinematical and dynamical quantities of symbols which obey laws of calculation of a novel type. There is also from the point of view an interesting formal analogy between quantum mechanics and the theory of relativity, in that it has been possible in both cases with the help of abstract concepts of arithmetic and geometry respectively, to build up strictly logical formalisms which allow a mastering of the new domains of experience. In connection with the often discussed question whether such formalisms can be regarded as an extension of our power of visualization, it must not be forgotten that the representation of the coördination of space and time in the theory of relativity by a four dimensional manifold, as also the connecting of kinematic and dynamic quantities in quantum mechanics by non-commutative algebra, rest essentially on the old mathematical artifice of the introduction of imaginary quantities; in fact the fundamental constants, the velocity of light and the quantum of action, are introduced into the formalism as factors of the $\sqrt{-1}$, the one in the definition of the fourth coördinate, the other in the commutation laws of canonically conjugate variables.

It is of course not my intention here to go deeper into such special points; I wished only to emphasize that in these fields the logical correlations can only be won by a far-reaching renunciation of the usual demands of visualization. It would in particular not be out of place in this connection to warn against a misunderstanding likely to arise when one tries to express the content of Heisenberg's well known indeterminacy relations—which play as important a rôle in the judgment of the consistency of the essentially statistical mode of description of quantum mechanics as the Lorentz transformation does in solving the paradoxes which appear in the theory of relativity—by such a statement as: "the position and momentum of a particle cannot simulta-

neously be measured with arbitrary accuracy." According to such a formulation it would appear as though we had to do with some arbitrary renunciation of the measurement of either the one or the other of the two well-defined attributes of the object, which would not preclude the possibility of a future theory taking both attributes into account on the lines of the classical physics. From the above considerations it should be clear that the whole situation in atomic physics deprives of all meaning such inherent attributes as the idealizations of classical physics would ascribe to the object. On the contrary, the proper rôle of the indeterminacy relations consists in assuring quantitatively the logical compatibility of apparently contradictory laws which appear when we use two different experimental arrangements, of which only one permits an unambiguous use of the concept of position, while only the other permits the application of the concept of momentum defined as it is, solely by the law of conservation.

We thus see that the impossibility of carrying through a causal representation of quantum phenomena is directly connected with the assumptions underlying the use of the most elementary concepts which come into consideration for the description of experience. In this connection the view has been expressed from various sides that some future more radical departure in our mode of description from the concepts adapted to our daily experience would perhaps make it possible to preserve the ideal of causality also in the field of atomic physics. Such an opinion would, however, seem to be due to a misapprehension of the situation. For the requirement of communicability of the circumstances and results of experiments implies that we can speak of well defined experiences only within the framework of ordinary concepts. In particular it should not be forgotten that the concept of causality underlies the very interpretation of each result of experiment, and that even in the coördination of experience one can never, in the nature of things, have to do with well-defined breaks in the causal chain. The renunciation of the ideal of causality in atomic physics which has been forced on us is founded logically only on our not being any longer in a position to speak of the autonomous behavior of a physical object, due to the unavoidable interaction between the object and the measuring instruments which in principle cannot be taken into account, if these instruments according to their purpose shall allow the unambiguous use of the concepts necessary for the description of experience. In the last resort an artificial word like "complementarity" which does not belong to our daily concepts serves only briefly to remind us of the epistemo-

logical situation here encountered, which at least in physics is of an entirely novel character (A_E).

The repeatedly expressed hopes of avoiding the essentially statistical character of quantum mechanical description by the assumption of some causal mechanism underlying the atomic phenomena and hitherto inaccessible to observation would indeed seem to be as vain as any project of doing justice to the increased profundity of the picture of the world achieved by the general theory of relativity by means of the ordinary conceptions of absolute space and time. Above all such hopes would seem to rest upon an underestimate of the fundamental differences between the laws with which we are concerned in atomic physics and the every day experiences which are comprehended so completely by the ideas of classical physics. Not only is the well known dilemma between the corpuscular and undulatory character of light and matter avoidable only by means of the viewpoint of complementarity, but the peculiar stability properties of atomic structures which are in obvious contrast with the properties of any mechanical model, but which are so intrinsically connected with the existence of the quantum of action, form the very condition for the existence of the objects and measuring instruments, with the behavior of which classical physics is concerned. On closer consideration, the present formulation of quantum mechanics in spite of its great fruitfulness would yet seem to be no more than a first step in the necessary generalization of the classical mode of description, justified only by the possibility of disregarding in its domain of application the atomic structure of the measuring instruments themselves in the interpretation of the results of experiment. For a correlation of still deeper lying laws of nature involving not only the mutual interaction of the so-called elementary constituents of matter but also the stability of their existence, this last assumption can no longer be maintained, as we must be prepared for a more comprehensive generalization of the complementary mode of description which will demand a still more radical renunciation of the usual claims of so-called visualization.

I hope by these remarks to have conveyed the impression that in abandoning the causal description in atomic physics we are not concerned with a hasty assertion of the impossibility of comprehending the wealth of phenomena, but with a serious effort to account for the new type of laws here encountered in conformity with the general lesson of philosophy regarding the necessity of a balance between analysis and synthesis. Just in this connection it appeared to me to be of interest to point out that also in other regions of human knowledge we meet appar-

ent contradictions which might seem to be avoidable only from the point of view of complementarity. I am far from sharing, however, the widespread opinion that the recent development in the field of atomic physics could directly help us in deciding such questions as "mechanism or vitalism" and "free will or causal necessity" in favor of the one or the other alternative. Just the fact that the paradoxes of atomic physics could be solved not by a one sided attitude towards the old problem of "determinism or indeterminism," but only by examining the possibilities of observation and definition, should rather stimulate us to a renewed examination of the position in this respect in the biological and psychological problems at issue.

In the first place, regarding the question of the extent to which we can hope to explain the characteristic features of living organisms with the sole help of the experience acquired from the study of inanimate nature, we must above all keep in mind that even a definition of life itself contains epistemological problems. When we usually refer to a machine as dead, we mean scarcely anything else than that we are able to describe the circumstances essential for its functioning by means of the conceptions of classical physics. Still in view of the insufficiency of the classical mode of description in atomic physics, such a definition of the inanimate would hardly any longer be adequate. Yet the newly recognized possibility of inducing macroscopic effects by individual atomic process, which plays an essential part in the functioning of organisms—in any case for the sensitiveness of sense perceptions (Aiv)—has been an incentive to the taking up anew of the question of a possible "explanation" of life. But at the same time the recognition of the fact that we must descend to the domain of atomic phenomena if we wish to bridge the gulf between the living and the inanimate, should bring before our eyes in a forceful way the practical and conceptual difficulties connected with this problem.

So far as we are at all in a position to follow the behavior of atoms in organisms under similar conditions of investigation as in the fundamental experiments of atomic physics, of course we can only meet with the laws disclosed by these experiments which, in spite of their feature of individuality, foreign to classical mechanics, can give us clearly no immediate understanding of the so-called holistic or finalistic characteristics of the activities of life. The only logical possibility of avoiding any contradiction between the formulation of the laws of physics and the concepts suitable for the description of the phenomena of life ought therefore to be sought in the essentially different character of the condi-

tions of investigation concerned. On a previous occasion (B) I have tried to express this situation by saying that every experimental arrangement suitable for following the behavior of the atoms constituting an organism in as exhaustive a way as implied by the possibilities of physical observation and definition would be incompatible with the maintaining of the life of the organism. This would in fact be quite analogous to the circumstance that all observations obtained by experimental arrangements which allow of a space-time account of the behavior of the constituents of atoms and molecules stand in a complementary relation to those obtained under conditions permitting the study of the intrinsic stability of atomic structures so essential for the physical and chemical properties of matter.

To make this view clearer, it was pointed out in the article cited, that the continuous metabolism of organisms inseparably connected with life prevents us even from distinguishing strictly which atoms belong to a living organism, and that we are thus presented with a problem the treatment of which, quite apart from its complication, is beyond the scope of the methods of atomic mechanics. These methods, which govern our entire knowledge of physics and chemistry concern, just as do those of classical mechanics, in fact only systems for which it is possible in principle to specify what are to be regarded as the elementary constituents. This situation suggests that those essential features of living organisms which are brought to light only under circumstances which exclude an exact account of their atomic constituents are laws of a nature which stands in a complementary relationship to those with which we are concerned in physics and chemistry. Thus the existence of life itself would have to be regarded in biology, both as regards the possibilities of observation and of definition, as no more subject to analysis than the existence of the quantum of action in atomic physics.

I have endeavored to make it clear that in such considerations there is no question whatever—as has been sometimes feared by philosophers and biologists—of so-called purely metaphysical speculations or of an arbitrary renunciation of the possibility by continued research, of further increasing our knowledge of the functioning of organisms. Rather, they aim at avoiding futile controversies by an analysis of the presuppositions and of the appropriateness of the conceptual structures involved. Though the viewpoint of complementarity rejects every compromise with any anti-rationalistic vitalism, it ought at the same time to be suited for revealing certain prejudices in so-called mechanism. On the one hand, any violation of physico-chemical laws in organic

life—such as the often mistakenly maintained contradiction between the activities of life and the fundamental theorems of thermo-dynamics—would be excluded from this point of view; on the other hand any insistence on an analogy between the existence of life itself and such laws should be rejected as irrational. As already emphasized in the article mentioned, this situation therefore implies no limitation whatever in the application to biology of the physico-chemical methods of description and investigation; in fact, the appropriate use of such methods—just as even in atomic physics all our experiences must rest upon experimental arrangements classically described—remains our sole and inexhaustible source of information about biological phenomena.

According to its tendency to make room for the phenomena of life within the conceptions suited to the description of material systems, the viewpoint discussed stands far removed from every attempt to exploit in a spiritual sense the failure of causal description in atomic physics. On the contrary, the viewpoint toward fundamental biological questions which we have here discussed, would rather seem suited to put the old problem of psycho-physical parallelism in a new light. The considerations which I have presented on previous occasions (A_{III}, B) on questions of psychology in connection with problems of atomic physics followed indeed two essentially different aims. The one was by means of well-known examples of the difficulties of analysis and synthesis of psychic phenomena connected with introspection to remind ourselves that in this region of knowledge we had already been forced to face a situation presenting in several respects a formal similarity with that with which,to the great disquietude of many physicists and philosophers, we have met in atomic physics. The other aim was to express the hope that the epistemological attitude which had led to the clarification of the much simpler physical problems could prove itself helpful also in the discussion of psychological questions. In fact, the use which we make of words like "thought" and "feeling," or "instinct" and "reason" to describe psychic experiences of different types, shows the existence of characteristic relationships of complementarity conditioned by the peculiarity of introspection. Above all, just the impossibility in introspection of sharply distinguishing between subject and object as is essential to the ideal of causality would seem to provide the natural play for the feeling of free will.

I am afraid that the short indications to which I have been obliged to restrict myself with respect to the last and many other points of this lecture will remind you only too well that in the last resort the direct

use of any word must stand in a complementary relationship to an analysis of its meaning. I hope, however, that I have to some extent succeeded in giving you the impression that my attitude is in no way in conflict with our common endeavors to arrive at as great a unification of knowledge as possible by the combating of prejudices in every field of research.

Copenhagen.

The New Logic

BY

KARL MENGER

(*Translated by H. B. Gottlieb and J. K. Senior**)

THE rapid development of physics, the result of observations made and ideas introduced within the last few decades, has brought about a change in the whole system of physical concepts. This fact is common knowledge, and has already attracted the attention of philosophers. It is less well known that geometry too has had its crises, and undergone a reconstruction. For centuries, so-called "geometrical intuition" was used as a method of proof. In geometrical demonstrations, certain steps were allowed because they were "self-evident," because the correctness of the conclusion was "shown by the adjoined diagram," etc. A crisis occurred in geometry because such intuition proved to be untrustworthy. Many of the propositions regarded as self-evident or based upon the consideration of diagrams turned out to be false. And so Euclidean geometry was reconstructed by methods free from all intuitive elements and strictly logical in nature. Moreover, for more than a century, various other geometries have been devised purely as logical constructions. Since they start with assumptions different from those of Euclid, and lead to conclusions partly in contradiction with his theorems, they are called "non-Euclidean." Nevertheless, each one of these geometries is a closed system of

* *Translator's Note:* This paper is a revised version of a lecture delivered in 1932 and published in "Krise und Neuaufbau in den exakten Wissenschaften." (Deuticke, Vienna, 1933.)

propositions exempt from contradiction. Recently, some of them have even found application in physics.

As far as experience and intuition are concerned, anyone, even though he be unfamiliar with the details of physics and geometry, can well understand that new empirical discoveries may overthrow the most venerable of ancient theories, and that intuition, if it ventures too far afield, may be forced to retire from positions erroneously occupied. One subject, however, is generally supposed to be unchanging and unshakable. That subject is logic. Hence, to anyone who is not an adept in the field, the recent active discussion of crisis and reconstruction within the realm of logic may seem not only strange but incomprehensible.

As a matter of fact, for two thousand years, logic has been the most conservative of all the sciences. Aristotle, who is considered the father of the subject, assumes as a starting point that every proposition ascribes a predicate to some subject. The propositions are classified on the one hand as affirmative or negative, on the other hand as universal or particular. "All cats are mammals" is universal and affirmative; "Some mammals are cats" is particular and affirmative; "Some mammals are not cats" is particular and negative; "No cat is a fish" is universal and negative. According to Aristotle, all inference consists in deducing a third proposition from two propositions of the given form. For example, from the premises that all cats are mammals and all mammals are vertebrates, it follows that all cats are vertebrates. From the premises that all cats are mammals and no mammal is a fish, it follows that no cat is a fish. All the inferences which Aristotle considered possible he arranged in three figures divided into altogether fourteen varieties. In the Middle Ages, these modes of inference were expanded to four figures including all together nineteen varieties, and were designated by the names Barbara, Celarent, etc. If, from the premises that to every (resp. no) M belongs the predicate P, and that to every S belongs the predicate M, it is inferred that to every (resp. no) S belongs the predicate P, then the inference is said to be according to the mode Barbara (resp. Celarent). Moreover, the three principles of identity, contradiction and excluded middle, which

were later called the fundamental principles of logic, were also formulated by Aristotle, curiously enough not in his logic but in his metaphysics. The kernel of his system, the above mentioned logic of subject-predicate propositions, was essentially all that was regarded as pure logic for two thousand years thereafter.

In the Middle Ages, it is true, the scholastics undertook a number of important logical investigations. But more recently, and particularly during the period of the Enlightenment, it became customary to regard the Labors of the scholars in the Middle Ages as hair-splitting; their work in logic was thus forgotten and was replaced by less fundamental considerations. Similarly, Leibniz's views on logic, which were far in advance of their time, also remained without direct effect. It was perfectly clear to Leibniz that a mere treatment of subject-predicate propositions was insufficient, and must be supplemented by a logic of relations. Furthermore, he treated the logical principles and their mutual relations more systematically than his predecessors, and devised the project of a "lingua characteristica," which should permit all scientific propositions to be stated in precise form, and of a "calculus ratiocinator," which should contain and treat mathematically all the methods of inference. But the clearest evidence that Leibniz's views, like the works of the scholastics, found no echo, is the famous dictum of Kant in the introduction to the second edition of the "Critique of Pure Reason." "That logic has trodden this sure path since the earliest times, can be seen from the fact that, since Aristotle, it has not been obliged to take a single backward step.—A further noteworthy fact is that until the present it has been able to take no step forward, and so seems apparently to be finished and complete." And in Kant's "Logic" occurs the statement "The logic of to-day is derived from Aristotle's Analysis.—Moreover, since Aristotle's time, logic has not gained much in content, and from its very nature it cannot. For Aristotle omitted no item of reason."

The first of the crises in logic was caused by defects in this Aristotelion system which were brought to light by mathematics. When confidence in geometrical intuition had been shaken, there

came a purely logical reconstruction of geometry. This consisted of a complete enumeration of all the hypotheses (axioms) concerning fundamental concepts not explicitly defined, and a deduction of the whole system of theorems from these axioms by strictly logical methods without any appeal to experience, diagrams, self-evidence or intuition. An analogous purely logical construction of arithmetic followed. This trend in mathematics naturally carried within a search for clarity in regard to all the principles of inference used in the deduction of mathematical propositions from axioms. And in the course of this search, it became evident that the old logic was utterly insufficient for the demands of modern mathematics in respect to both precision and completeness. It was therefore chiefly mathematicians who undertook and carried out the desired reconstruction of logic. A short sketch of the chief stages in this reconstruction (which is called logistics) will next be given; but it should be understood that this logistics, although much more far-reaching than the Aristotelian logic, is to-day counted as part of the classical content of logic. It will appear that the real problems of the new logic only begin where logistics stops.

The first step in the reconstruction was the development of the so-called calculus of classes (also called algebra of logic) particularly by Boole, Pierce and Schröder in the second half of the last century.[1] The Aristotelian logic, as stated, deals mostly with the question: If the relations of two classes to a third class are known, what can be said about the relations of the two classes to each other? For example, if the relations of the class of all cats and the class of all vertebrates to the class of all mammals are known, what can be said of the relations of the class of all cats to the class of all vertebrates? The calculus of classes is more extensive; for it is a systematic theory of the relations of any number of classes. Aristotle only investigates inclusions. He asks whether of two classes one is entirely or partly contained in

[1] See Boole, "The Mathematical Analysis of Logic," Cambridge (1847), and "An Investigation of the Laws of Thought," London (1854) and Chicago (1916); Schröder, "Vorlesungen über die Algebra der Logik," 3 vols. Teubner, Leipzig (1890–1910) and "Abriss der Logik," ibid. (1909–1910). See also the first chapters of Lewis and Langford "Symbolic Logic," The Century Company, New York.

the other, or whether one is entirely or partly outside the other. But the calculus of classes investigates many other relations between classes besides inclusion, and undertakes many other operations with classes. Among other things, it treats systematically of the "join" and "intersection" of two classes A and B—that is the class of all elements in either A or B as well as the class of those in both A and B. In contradistinction to Aristotle, the null class (containing no member) is also considered. For instance, the intersection of the class of all cats and the class of all fish is the null class. The calculus of classes, starting from a certain few propositions, gives a systematic treatment of the relations of all classes. Among its theorems, there are nineteen which correspond to the Aristotelian scholastic modes of inference.[2]

But in the calculus of classes, these nineteen theorems are not the only ones; moreover they occupy in it no distinguished position. They are not among its initial propositions (in the first place because the whole of this calculus can be based on much fewer than nineteen propositions), and so they are not necessary for the founding of a systematic calculus of classes. But even if it were desired to assume so needlessly large a number of initial propositions, the particular nineteen Aristotelian ones would not be sufficient for the construction of the whole calculus of classes. This calculus is therefore a definite advance over the old logic of class inclusion.

A second step leads further. The calculus of classes is a theory deduced from a few initial propositions concerning the relations of classes, just as Euclidean geometry is deduced from a few initial propositions (axioms) concerning the relations of points,

[2] According to Ladd Franklin, these can moreover be combined into a single theorem, namely the theorem that for the three classes A, B and C, it cannot be simultaneously true that the intersection of A and B is the null class, the intersection of not-B and C is the null class, and the intersection of A and C is not the null class. In the case of the mode Barbara, the premises are: All M is P and all S is M. That is, the intersection of the class M with the class not-P is the null class, and the intersection of the class S with the class not-M is the null class. Then, according to the Ladd Franklin formula, it is impossible that the intersection of the classes S and not-P should not be the null class. In other words, the intersection of the class S and the class not-P is the null class. That is, all S is P, and the assertion of the mode Barbara is thereby proved.

lines, and planes.[3] The calculus of classes is thus a special mathematical theory; it is, however, far from containing the whole of logic, for logic does not confine itself to the consideration of classes. If further propositions are deduced from any set of propositions, this deduction is called logical inference. The subject matter of this inference is, however, not classes but propositions. And yet logic is expected to deal with the rules of inference. The second step in the expansion of logistics, which goes back principally to Pierce and Schröder, was therefore the development of a calculus of propositions. This calculus teaches how propositions are combined by words like "and," "or," "not" and similar particles.[4] If p and q are two propositions, then the propositions (not-p), (not-q), (p or q) and (p and q) are also considered. Particularly important is the relation "q or not-p" which is briefly expressed in logic by the words "p implies q" or "q is implied by p." If q is true, then the proposition "q or not-p" is surely true, whether p is true or false. According to the given terminology, a true proposition q is therefore implied by every proposition. If p is false, then not-p is true, and the proposition "q or not-p" is surely true whether q is true or false. Thus a false proposition p implies every proposition q.

Of special importance among the propositions composed of several statements connected by logical particles are those which are true in all cases, whether the component statements are true or false. For example, the proposition "p or not-p" (It is raining or it is not raining.) is true, whether the component statement "p" (It is raining.) is true or false. At Wittgenstein's suggestion,

[3] The calculus of classes is not only methodologically the same as an axiomatic geometry. It can, in fact, as I have on occasion remarked, be brought formally under one heading with elementary geometrical theories. That is, there exists a theory including elementary geometry and the calculus of classes. From this theory, each of the two more special theories may be obtained by specific additional axioms. Cf. Jahresbericht der deutschen Mathematiker-Vereinigung, *37*, 309 (1928), and Annals of Mathematics, *37*, 456 (1936).

[4] More detailed expositions of the calculus of propositions as well as of the calculus of functions are to be found in Frege, "Begriffsschrift," Halle (1879); Whitehead-Russell, "Principia Mathematica," Cambridge University Press (1925); Hilbert-Ackermann, "Grundzüge der Theoretischen Logik, Springer, Berlin (1928); Carnap, "Abriss der Logistik," Springer, Vienna (1929).

such complex propositions which are always true are called tautologies. The calculus of propositions is concerned with the setting up of tautologies, and it follows from Frege's work that all tautologies can be deduced from a few simple tautologies.

It may be thought that the calculus of propositions treats only those transformations which result from the applications of the three Aristotelian principles (identity, contradiction and excluded middle) to propositions. Such, however, is not the fact. More correctly, the three Aristotelian principles play a rôle in the calculus of propositions similar to that played by the Aristotelian modes of inference in the calculus of classes. That is, the principles of identity, contradiction and excluded middle occur among the theorems of the calculus of propositions, but they are not the only theorems of this calculus; and moreover they enjoy in it no specially distinguished position. In particular, they do not appear among those propositions actually chosen as initial propositions for this calculus; thus they are not a necessary part of its foundation. Moreover, they are not sufficient for the deduction of the whole of it. The calculus of propositions is thus a decided advance over the old logic.

Since the whole calculus of propositions can be deduced from certain simple initial propositions, and since, on the other hand, the principles of this calculus are supposed to be the principles of logical deduction, it might perhaps be suspected that the calculus in question is founded on some sort of reasoning in a circle. Frege, however, carefully avoided this danger. The initial propositions in question are merely certain propositional formulae—in fact tautologies. According to Lukasiewicz, the three following which involve the two undefined concepts "not" and "implies" may be chosen for this purpose.

(1) p implies (not-p implies q).

(2) (not-p implies p) implies p.

(3) (p implies q) implies [(q implies r) implies (p implies r)].

Of these, the first corresponds to the previously mentioned condition that a false proposition implies every proposition. The second corresponds in a certain sense to the law of double negation; for, starting with "or," the proposition "not-p implies p"

stands for "p or not-not-p," and the second formula stands for "not-p or (p or not-not-p)." The third formula is closely related to the syllogism; if q is implied by p, and r is implied by q, then r is implied by q.

The calculus of propositions is the totality of those propositional formulae which can be obtained from the three initial formulae by the use of certain formative rules. These are simply the following. First, in the initial formulae or in formulae already obtained from them, the symbols p, q and r may be replaced by other and possibly complex propositional symbols. For example, if, in the first initial formula, "r implies s" is substituted for p, there is obtained the formula "(r implies s) implies [not (r implies s) implies q]" which, according to the first formative rule, is to be taken into the calculus of propositions. Second, if the proposition p and the proposition "p implies q" are two formulae in the calculus of propositions, then q is also admitted as a formula of this calculus. It is thus clear that the calculus in question and the inference used for its systematic development are kept carefully separate. The calculus consists of formulae; the development of the calculus is accomplished by the use of two rules for the construction of formulae. Both the initial formulae and the two formative rules are precisely defined. Obviously the two latter are considerably simpler than the usual rules of logical inference.

But even a combination of the calculus of propositions with the calculus of classes does not exhaust the content of logic. Most common propositions, particularly those of mathematics, besides employing such words as "and," "or," "not" or "implies," contain other logical particals, especially "all," "some" or "there are." The exact rules for dealing with propositions containing these so-called logical quantifiers forms a third chapter of the newer logic which, since the work of Pierce and Frege, has taken its place beside the calculi of classes and of propositions; it is called the calculus of functions. The historical origin of this name is as follows. Besides propositions, some of which are true like "This charcoal is black," and some of which are false like "This charcoal is red," there are also word combinations like

"x is black" which are not propositions, but which become propositions only when the name of a definite individual within a certain field is used to replace the symbol x, or when the combination is preceded by a quantifier. Such word combinations are called propositional functions. For example, the propositional function "x is black" becomes a true proposition if "this charcoal" is substituted for x. It becomes a false proposition if "this lime" is substituted for x. When mankind is chosen as the range of x, the proposition becomes false if the quantifier "all" is placed before x, for then it becomes the false proposition "All men are black." Finally, if the quantifier "some," is placed before x, the proposition becomes the true existence proposition "Some men are black." Since the rules for logical operations on general and existence propositions are deduced from the theory of propositional functions, the study of operations with such propositions is called the calculus of functions.

The calculi of classes and of propositions together with the hitherto considered portions of the calculus of functions may be interpreted as mere refinements on the old logic—if the word "refinement" is used in a very broad sense. But the fourth step in the development of the new logic is undoubtedly an extension of the content of the subject. The impetus to this step also came from mathematics; for the propositions which are the subject of mathematics, and which were first expressed by Peano in a general and rigorous symbolism, are only rarely statements about the membership of individuals in classes or about inclusions between classes. Neither are they often propositions consisting of statements about classes connected by the words "and," "or," "not," "implies," "all" or "some." Most mathematical propositions deal rather with relations, as Leibniz already recognized. The proposition "3 is less than r" states a relation between two numbers; the proposition "If, on a straight line, the point q lies between the points p and r, then r does not lie between p and q" is a general statement about a relation between the members of triples of points on straight lines. A logic useful to mathematicians must above all treat of relations.

A predicate corresponds to a class, namely the class of all those

things which have that predicate. For instance, the predicate black corresponds to the class of all black things. Similarly, a relation between two things (called a dyadic relation) corresponds to a class of pairs of things, namely the class of all those pairs of things in which the first member of the pair has the given relation to the second. For example, the relation "less than" corresponds to the class of all pairs in which the first member is less than the second. The extension of logic which treats propositions on relationship along with subject-predicate propositions can thus be characterized by the fact that besides classes of individuals, it investigates classes of pairs of individuals, classes of triples of individuals, etc. This subject also starts with certain initial formulae, and, by the help of a few exactly defined rules, deduces from them a system.

Although logic is thus greatly increased in content, it is still insufficient to account for all the conclusions drawn in modern mathematics. In order to follow the newest mathematics, particularly the theories of real numbers and of sets, a fifth step had to be taken. It was necessary to create an expanded calculus of functions which deals with classes of all sorts of classes of individuals, with classes of classes of classes, etc. This extremely important extension of logic is not only necessary to account for all of modern mathematics, but according to Russell, is also sufficient (when taken in connection with the portions of logic previously discussed) to serve as a basis for all of that science. That such is the fact will next be shown in a short sketch.[5]

Once in possession of this expanded calculus of functions, it is possible to define when two classes A and B are equinumerate or of the same power. Since Georg Cantor's day, they have been thus designated when they can be placed in one to one correspondence, with one another—that is, when to every member of the class A, a member of the class B can be assigned in such fashion that each member of B is assigned to just one member of

[5] An easily comprehensible exposition of the logical foundations of mathematics is Russell's "Introduction to Mathematical Philosophy," Allen and Unwin, London (1920).

A. For example, the classes of points and crosses here shown

are said to be of equal power because the class of points can be placed in one to one correspondence with the class of crosses by assigning each point to the cross directly above it. On the other hand, the classes of points and stars are not of equal power, for, no matter how a star is assigned to each point, there will still be a star which is assigned to more than one point. Children and savages count low-numbered classes by bringing the members of the class to be counted into one-to-one correspondence with the members of a class of the fingers of their hands. This fact apparently accounts for the importance of the number ten in the common number system.

It should not be imagined that the concept of equality of number presupposes a concept of number or an operation of counting. Without counting how many auditors or seats there are in a lecture room, the number of auditors and the number of seats may be shown to be the same by ascertaining that each seat is occupied and that each auditor has a seat; by these facts, the one-to-one correspondence necessary and sufficient for equality of number is established between the class of all auditors and the class of all seats. In the theory here outlined the concept of number depends on the concept of equality of number, for a number is defined as the class of all classes which are equinumerate with a given class. For instance, five is defined as the class of all those classes (such as the classes of crosses and points shown above) which are equinumerate with the class of the fingers of one hand. But Cantor went further. For each infinite set S, he combined into a class all those sets which are of the same power as S. One of his great discoveries on which he founded the theory of sets is that various infinite numbers can be obtained in this way, for not every infinite set is of the same power as every other infinite set.

Among all the numbers thus obtained, the finite or natural numbers (1, 2, 3 . . .) are defined simply by the application of the logical concepts. The definition is based essentially on the fact that the important principle of complete induction holds for these numbers. This principle is that any proposition which holds for the number one, and which, if it holds for any natural number n, also holds for the succeeding number n + 1, holds for every natural number. A proposition valid for all natural numbers and proved by the principle of complete induction is that every natural number can be expressed in one and only one way as a product of prime numbers. This principle of complete induction has shown itself to be one of the most important methods of proof in arithmetic.

Within the field of the natural numbers, addition and multiplication can be carried on without restriction, but subtraction and division cannot always be performed. That is, to every pair of natural numbers a and b there corresponds a natural number a + b as their sum and one ab as their product, whereas natural numbers a − b and a/b exist only if b is respectively smaller than or a divisor of a. In order to make subtraction possible for any two numbers, a new calculus of pairs of natural numbers is introduced. The pairs (a, b) and (c, d) are called equal if and only if a + d = b + c. Addition and multiplication of such pairs are defined by the formulae

$$(a, b) + (c, d) = (a + c, b + d)$$
$$(a, b) \times (c, d) = (ac + bd, ad + bc)$$

Development of this theory shows that the properties of addition and multiplication are the same among such pairs as in the field of the natural numbers. The pairs (0, 0), (1, 0), (2, 0) . . . act in all respects like the natural numbers 0, 1, 2 . . . . The pairs (0, 1), (0, 2), (0, 3) . . . may be briefly designated as −1, −2, −3 . . . since (a, 0) + (0, a) = (a, a) = (0, 0) . . . an equation which corresponds to the equation a + (−a) = 0.

The calculus is constructed so that the integer g is defined as the set of all pairs of natural numbers (a, b) such that b + g = a if g is positive, and b + (−g) = a if g is negative. In an analogous way, rational numbers (fractions) are introduced as pairs

of integers by another new calculus which permits unlimited performance of addition, subtraction, multiplication and division (except division by 0). Briefly stated, integers are introduced as differences between natural numbers; rational numbers are introduced as quotients between integers. These rational numbers can be ordered according to their magnitudes. The quotient g/h is called greater, equal to or less than the quotient g'/h' if and only if gh' is respectively greater, equal to or smaller than $g'h$. Between any two fractions a/b and c/d a fraction of inter mediate size for example $(a + c)/(b + d)$ can be interpolated.

Nevertheless there remain gaps between the fractions. These are associated with the fact that, in the field of fractions, addition, subtraction, multiplication and division (except division by 0) can be carried on without restriction, but other operations, for example the extraction of roots cannot always be performed. It is easy to show, for instance, that there is no fraction a/b such that its square a^2/b^2 is equal to 2, a fact which is also expressed by the statement that $\sqrt{2}$ is not a rational number. However there are fractions with squares arbitrarily close to but less than 2 and fractions with squares arbitrarily close to but greater than 2. Between these two classes of fractions (the lower class consisting of all those with squares less than 2, and the upper class consisting of all those with squares greater than 2) there is a gap in the class of all fractions, for a fraction with a square equal to 2 does not exist. In order to fill out such gaps, and in order to make possible a large number of other operations (like extraction of roots, solution of all equations of odd order, etc.) the real numbers are introduced as a certain kind of division of the class of all fractions into two sub-classes. Those divisions corresponding to gaps in the class of fractions are called irrational numbers. As soon as all the real numbers have been introduced, it is at last possible to define logically the notion of limit so fundamental for all higher mathematics, and thus to define logically the notions of continuity and the differentiation or integration of functions, as well as the concept of n-dimensional space which underlies analytic geometry. This last is defined as the class of all n-tuples of real numbers. By another new calculus with pairs of real numbers, the complex numbers are introduced. Then,

according to the fundamental theory of algebra, all equations (in particular the equation $x^2 = -1$) become solvable, and every equation of the n-th degree has n roots.

In a word, after logic has been provided with the expanded calculus of functions, mathematics, as Russell says, becomes a part of logic. This assertion is not by any means an arbitrary extension of the word logic to include all of mathematics. The historical development has just been described. All the conclusions drawn by modern mathematics were treated just as the most primitive conclusions were treated in the calculus of propositions. In other words, all these conclusions were derived from certain initial formulae by the help of certain formative rules. Thus there were obtained initial formulae which not only permit the deduction of mathematical conclusions, but also suffice for the derivation of all mathematics.

Such was the culmination of logistics, which at this point contained all that was needed. After two thousand years of petrification, logic had, in less than half a century, been entirely reconstructed by the mathematicians; and in the year 1900 an article on logic might have terminated with this happy result.

About the turn of the century, however, there came an entirely unexpected repercussion of the worse kind, and a second crisis in logic, a crisis in logistics occurred; for the newly introduced method of unlimited operation with classes and classes of classes led to nothing less than an antinomy. Now an inner contradiction is unbearable even in the special theory of a particular science. For logic, however, the appearance of a contradiction in its structure is catastrophic.

Before explaining the newly discovered antinomy, it should be mentioned that paradoxes had appeared even in ancient logic. The one about the liar is well known. It was called the Cretan inference, because the Cretans were supposed to be particularly mendacious. In ancient times, the statement "All Cretans are liars" in the mouth of a Cretan was regarded as a logical paradox. The best way to illustrate this sort of paradox with modern precision is to write on a board the following statement:

"The statement on this board is false."

Nothing further is written on the board, and the given statement is examined to see whether it is true or false. It will be proved to be neither. Assuming that the statement is true, it follows that the statement on the board is false, in contradiction with the assumption that the statement is true. Assuming that the statement is false, since this is precisely that the statement on the board, it follows that the statement on the board is true, in contradiction with the assumption that the statement is false. Thus the assumption that the statement is false leads to the conclusion that it is true; and the assumption that the statement is true leads to the conclusion that it is false. In other words, the assumption of either truth or falsehood for the statement leads to a contradiction. Hence, the statement is neither true nor false, which is of course paradoxical, for, according to the principle of the excluded middle, every statement is either true or false, and any third possibility is excluded.

As a matter of fact, precise analysis shows that the words "statement on this board" play an essential rôle in the paradox in which they appear. These words are not of a logical nature and, for this reason, the above and related paradoxes are (at Ramsay's suggestion) distinguished by a special name from purely logical antinomies and called epistemological paradoxes. According to a recent research by Tarski,[6] what such paradoxes really show is that a self-consistent use or correct definition of the term "true proposition" which is in harmony both with the laws of logic and with every-day language is impossible.

The severe crisis in logistics was brought on in 1901 by Russell's discovery (following some related results obtained by Burali-Forti) of a purely logical antinomy. Such an antinomy is one in which appear only the concepts of the logical calculus of functions, particularly the concept of class. If M is the class of all men, then

(a) Every member of M is a man.

(b) Every man is a member of M.

[6] Tarski, "Der Wahrheitsbegriff in den formalistischen Sprachen," Studia Philosophica, Lespoli (1935). This important paper contains the first successful treatment of a classical problem in philosophy by means of modern logic.

The class M itself is not a man but a class of men, and therefore, according to (a) does not occur among the members of M. Similarly the class of all triangles in a plane is not a triangle and therefore does not appear among its own members. As these and many other examples show, there certainly exist classes which do not occur among their own members. On the other hand, if N is the class of all non-men, then

(a) Every member of N is not a man.

(b) Everything that is not a man is a member of N.

As N itself is not a man but a class of non-men, according to (b), N occurs among the members of N. Another example of a class which occurs among its own member is the class of all classes. If this class is called K, then

(a) Every member of K is a class.

(b) Every class is a member of K.

As K itself is a class (namely the class of all classes), according to (b), K occurs among the members of K. Now let L be the class of all classes which do not occur among their own members. Then

(a) Every class which is a member of L does not occur among its own members. (For example, the classes K and N just mentioned are not members of L.)

(b) Every class which does not occur among its own members is a member of L. (For example, the class of all men M and the class of all triangles in a plane are members of L.)

The question at issue is whether or not the class L occurs among its own members. First: It is impossible for L to be a member of L. For if L were a member of the class L, then L would contain as a member a class (namely L) which occurred among its own members, whereas according to (a) every class which is a member of L does not occur among its own members. Second: It is impossible for L not to be a member of L. For if L were not a member of L, then L would be a class which did not occur among its own members and still was not a member of L, whereas according to (b) every class which does not occur among its own members is a member of L. Hence it is impossible for L to be a member of

L, and it is impossible for L not to be a member of L. This, however, is an antinomy, for, according to the principle of the excluded middle, every proposition is either true or false and, as has been shown, the proposition "L is a member of L" is neither true nor false.

The effect of Russell's discovery was enormous. Most of what has since been done in logic has been more or less influenced by the fear of antinomies. It will be shown that the importance of the three methods pursued by modern logic far transcends that of the antinomies themselves, but historically the latter have been the most important stimulus for the recent developments.

The first means of surmounting the crisis was found by Russell,[7] the discoverer of the purely logical antinomy which had caused the trouble. His solution is as follows: Every logical construction which occurs in mathematics must have as a starting point a certain field of individuals. Besides these individuals, there come under consideration classes of individuals, which should not be confused with the individuals themselves. Furthermore, there are classes of classes of such individuals (called classes of the second type) which are not to be confused with classes of individuals (called classes of the first type). In general, for every natural number n, there are classes of the n-th type, and all these classes of different type must be carefully distinguished. Particularly, in speaking of all classes, it must always be indicated whether all classes of the first, second or n-th type are meant. A class which contains as members classes of different types must not be formed. The class of all classes mentioned above is a concept which cannot occur if this prohibition is respected. It does not belong in the hierarchy of types. The class of all classes which do not occur among their own members is similarly forbidden, and so this concept which opens the door to antinomies is excluded by the type theory.

As a matter of fact, up to now, no antinomies have been discovered which are not excluded if the rules of the type theory are observed. But although this theory forbids certain concepts which have not led and probably will not lead to contradictions,

[7] Cf. particularly Russell's "Introduction" cited in (4).

it gives no assurance that all possible but still undiscovered antinomies are excluded by it. For this reason, Poincaré, under similar circumstances, spoke of a shepherd who, to protect his flock from wolves, built a fence around it, without, however, being sure that he had not enclosed a wolf within the fence.

A second program which has been followed since a logical crisis was precipitated by the discovery of antinomies consists in the development of a method which had already been vigorously pursued by the mathematician Kronecker in the 80's of the last century, even before the antimonies had been found. It has been called by his followers intuitionism.[8] This name is borrowed from philosophy where it is attributed to the methods initiated by Bergson. As will appear, there are really points of contact between the ideas of the two schools, though there are mathematician-intuitionists who sometimes deny this assertion.

Whereas Russell calls mathematics a part of logic, Kronecker takes his stand on the primacy of mathematics as opposed to logic. According to him, mathematical construction is fundamental. "The whole numbers were created by God; everything else is human handiwork." In particular, logical inference, if unaccompanied by mathematical constructions, can lead to mathematically false conclusions. Above all, Kronecker opposes indirect proofs. In these, it is proved that the non-existence of an entity with the given properties involves a contradiction. Hence the existence of an entity with the given properties is inferred, although no method for the construction of such an entity need be given. An example may illustrate this attitude. Suppose it were possible to deduce a contradiction from the assumption (called Fermat's hypothesis) that for every integer $n > 2$ and for every triple of integers x, y and z, the inequality $x^n + y^n \neq z^n$ subsists. The classical

[8] I gave an account of the historical development of this school of thought with many literature references in an article "Der Intuitionismus" Blätter f. deutsche Philosophie, 4, 311 (1930). More recent results are summed up by Heyting in "Ergebnisse d. Mathematik u. ihrer Grenzgebiete," vol. 3, Springer, Berlin (1934). An account of the discussions of foundation questions together with many literature references is to be found in Fränkel's "Mengenlehre," 3rd ed., Springer, Berlin (1928).

mathematician would express this fact in the following words: There exist four integers n (> 2), x, y and z such that $x^n + y^n = z^n$. Kronecker, on the other hand, would enunciate this existence theorem only if four such integers had been found, or at least if there had been given a method which would permit the finding of such integers in a finite number of steps. If it were proved that integers of the desired kind existed among those with 1000 digits, then such integers could certainly be found if at all the quadruples those with 1000 digits were examined successively to see whether or not they satisfied the equality $x^n + y^n = z^n$. This process would, it is true, be so tedious that no man could carry it out, but nevertheless it would lead to the goal in a finite number of steps. On the other hand, the mere proof of the impossibility of the non-existence of integers of the desired kind generally furnishes no procedure for the discovery of such integers. In this latter case, Kronecker denies that the existence of an integer of the desired kind has been proved.

Poincaré directed his attacks against what he called impredicative definitions—definitions of an entity which refer to classes in which the entity to be defined belongs. The class of all classes is a typical impredicative concept. As a matter of fact, the concepts of largest number and of an upper limit to a set of numbers (concepts which are important for all higher mathematics) are also impredicative. Hence Weyl in his book "The Continuum" (1918) concluded that these concepts are to be discarded save when the entities in question can be determined operationally —that is, when they can be fixed by a definition which is not impredicative.

Early in this century, the question whether the so-called "axiom of choice" formulated by Zermelo was to be accepted or rejected played a great rôle in the discussions of the Paris school. In fact this question caused more argument than the "axiom of comprehension" which permits arbitrary subsets of any set to be formed, although this latter axiom leads, I believe, to much greater difficulties than does the former, and in certain constructions even condenses all the difficulties into one. Complete

induction, on the other hand, is regarded as a valid conclusion by the Paris school. Poincaré considered it a synthetic, a priori judgment in the sense of Kant.

During the last two decades, Brouwer has followed out the consequences of Kronecker's theses, and has shown how much of modern mathematics rests on indirect proofs of existence, and falls with these proofs. Logic, according to him, gives only the rules of the language for mathematical inference about finite systems. To apply it to infinite systems (sequences, sets, etc.) is meaningless. In particular, the principle of the excluded middle, the source of the indirect proofs of existence, is not to be applied to infinite systems. Brouwer takes over Kronecker's restriction on the use of the words "there exists"; they are to mean "there can be defined in a finite number of steps." And so (as long as Fermat's hypothesis has been neither proved nor disproved) he denies the validity of the alternative: Either for every quadruple of integers n (> 2), x, y and z the inequality $x^n + y^n \neq z^n$ holds or there exist four integers n (> 2), x, y and z such that $x^n + y^n = z^n$. Only when the Fermat problem shall have been solved (that is when it shall have been proved either that if $n > 2$, $x^n + y^n \neq z^n$ for all integers, or a quadruple shall have been found such that $x^n + y^n = z^n$) . . . only then will it be seen a posteriori that one of the two possibilities maintained by the alternative actually occurs. As long as the Fermat hypothesis is neither proved nor disproved, not only is the solution unknown, but the very solubility of the problem is uncertain, so that a priori even the alternative cannot be asserted. On the other hand, it cannot be maintained a priori that the Fermat problem or any given problem is insoluble.

According to Brouwer, his own constructions, complete induction in particular, are based on an "original intuition" which, he says, also yields a concept of set. Investigations in the theory of sets which go beyond this concept he discards as meaningless. For instance, he adopts Bergson's view that continuity cannot logically be treated in the classic fashion as a completed aggregate of points.

This intuitionist program requires examination. Does it, first

of all, avoid all danger of antinomies? Intuitionists claim that their pure constructions can "obviously" never lead to a contradiction. As a matter of fact, a strong feeling of assurance attaches to those parts of arithmetic which are admitted by intuitionists; this feeling, however, lessens when it comes to their theory of sets. But anyone who, in his search for consistency, requires something more than feelings, or allusions to obviousness and intuition finds no help in intuitionism.

Moreover, the feeling that the intuitionist calculus of propositions and theory of numbers are more secure against contradiction than the corresponding classic theories can be proved to be illusory. The proof was recently established by Gödel. The erroneous belief arose from the fact that the whole intuitionist system corresponds to only a part of the classic results. But Gödel[9] recently found, conversely, that the whole classic calculus of propositions and theory of numbers (including the principle of the excluded middle) correspond to a part of the intuitionist theories, for every classical theorem within this field can be translated into an intuitionist theorem by the use of a simple dictionary. Were there a contradiction between any two classical theorems, there would also be one between the two corresponding intuitionist theorems. One of the rules for translation is that wherever, in the classical theorems, the words "p or q" appear, they must be replaced by the words "it is impossible that p is impossible and q is impossible." Hence, abandoning the principle of the excluded middle, but at the same time admitting (as intuitionists do) the impossibility of universal propositions, does not really limit but only renames the classical theorems in the calculus of propositions and the theory of numbers.

For the special concepts of the theory of sets, I some years ago devised a dictionary[10] by the use of which all the intuitionist concepts within this field may be translated into known special concepts of the classical theory. What Brouwer calls "sets," "individualized sets" and "species" correspond respectively to

[9] See Gödel in "Ergebnisse eines mathematischen Kolloquiums," *4*, 34, Deuticke, Vienna (1933).

[10] Jahresbericht d. deutschen Mathematiker-Vereinigung, *37*, 213 (1928).

"analytic sets," "Borelian sets" and "general subsets of analytic subsets" in the classical theory. Here the difference between the intuitionist and the classical developments lies only in the fact that the intuitionist dogmatically limits himself to the above mentioned special concepts. These he calls meaningful and constructive; more far-reaching ideas he designates as meaningless. But there is no proof of the impossibility of contradictions within the restricted theory. The only certainty is that it excludes large parts of classical mathematics.

How can classical mathematics be defended against these intuitionist attacks? This can be done by a sober analysis of intuitionism. Here two things are to be distinguished: the mental activity of the intuitionist mathematicians, and their verbal and written reports of this activity. The intuitionists, indeed, take the attitude that mathematics consists only of their own constructive mental activity, whereas all external communications are merely more or less imperfect directions for the repetition of this mental activity by other human beings. Naturally, a sober critic can do nothing but stick to their external communications. These are, in part, of a mathematical nature, and contain not only constructions but also proofs which employ certain methods of inference in a perfectly regular fashion; in part they belong rather to a theory of knowledge, and maintain that intuitionist ideas have meaning, whereas more far reaching methods of inference are meaningless.

First, as to assertions like "Certain methods of inference are based on intuition; others are meaningless." Such assertions are, in my opinion, nothing more than descriptions of subjective psychological processes or expressions of subjective tastes. They are therefore of interest only to biographers and historians; they do not belong to logic or mathematics. They are value judgments and thus at root expressions of feeling. According to one's own feelings they may be accepted or opposed by voluntary decisions concerning the methods of inference and construction actually to be used or not to be used. To my mind they recall the dietetic rules enunciated by certain philosophical schools.

Secondly, as to the residue of the intuitionist constructions when they have been purified and freed from their non-mathematical elements. This residue consists of the deduction of propositions by the help of certain methods and modes of inference; it is a system of inferences according to certain rules. But classical mathematics is nothing else. Intuitionists, to be sure, say that the whole of their mental constructions cannot possibly be formulated—cannot possibly be precisely stated in any system of axioms. Yet the inferences which the intuitionists, according to their verbal communications, actually draw, they draw with perfect regularity. These inferences can therefore be collected and traced back to a certain few principles. In fact, Heyting[11] has actually set up a system of axioms for this intuitionist calculus of propositions and of functions. Consequently, the claim of the intuitionists can only mean that they reserve the right to use other inferences besides those contained in any system of axioms. At any given moment, however, the inferences actually drawn by them can be formulated. For example, up to now, in the calculus of propositions and functions, they have drawn no inference which is not permitted by Heyting's system. But should they, because of new discoveries or changes in conviction, use additional methods of inference, then it would merely be necessary to expand the systems of axioms, and again a formal system representing what had actually occurred would be obtained. In intuitionism, what is formulated is, so to speak, limited at the bottom—that is, a certain minimum is assured; it is left open at the top—that is, no maximum is fixed. But similarly, classical mathematics and logic do not by any means insist on the complete immutability of their underlying axioms, and they too reserve the right to introduce new ones. From this point of view, the only difference between the two schools is that intuitionists, although they do not fix a totality of the modes of reasoning, a priori exclude certain modes for all time. In the sense of the simile used above, their dietetic prescription does not

[11] Heyting, Sitzungsberichte d. preussischen Akademie der Wissenschaften, pp. 42, 57, 158, (1930).

enumerate all proper foods. It consists of examples of things to be eaten, together with the prohibition of certain forms of nutriment.

The situation may be elucidated by considering the concepts "definability," "constructability," and "provability" which are discussed in intuitionism. All the applications of these terms and all the rules regarding them have not yet been precisely fixed, not even in Heyting's system. The insistence of the intuitionists on their own constructions and their renunciation of more far-reaching "unconstructive" arguments would seem to me of mathematical importance, particularly in the theory of sets, on one condition—that is, if there were only one way of defining the conditions for constructability, so that the mathematics meeting these conditions would be distinguished from all other possible mathematics. Such however, is certainly not the fact. The postulates for constructability may be varied by degrees. All the imaginable postulates may be combined in such varied ways that it is not even possible to arrange the different combinations in a linear order. There are systems which partly overlap one another. Incidentally the same is true of diets. One code forbids flesh, a more restrictive one interdicts eggs also; a third, overlapping the second, permits eggs but neither flesh nor fish, etc. Observation shows that intuitionists sometimes differ as to what they allow and what they forbid, and even the same intuitionist may change his mind from time to time. Compare the theory of Lusin (based upon the views of Borel, Lebesgue, et al.) with that of Brouwer, or contrast the various stages of Brouwer with one another. There is no question that these various theories have a certain interest, but their interpretation by their respective authors is, I believe, untenable.

What seems to me of importance, what I have for years tried to point out, and what I have attempted to sum up in the foregoing remarks are the following facts:

(1) All that any intuitionist does is to realize one out of a great variety of possible conditions which can partially be arranged in order of stringency, but which partially overlap one another.

(2) Were the intuitionist to omit the statement that his theory is the one founded on intuition whereas all more far-reaching arguments transcend intuition, he would lose no mathematical advantage. Such dicta are only weak points open to attack, for they introduce nothing but psychological elements, personal attitudes and value judgments.

(3) It is certainly possible to construct mathematical systems more restricted than any one so far developed. The negation of universal propositions might be excluded, or, after elementary steps of control had been defined, only conclusions open to control by a limited number (say a million) steps might be admitted. Perhaps some one will some day succeed in stating precisely what is meant by a finitistic mathematics; for this task, in spite of much talk, has not yet, so far as I can see, been accomplished. Another possibility would be the development on the basis of modern logic, of a mathematics suitable for physics, a problem of great importance for this latter science.

The third program followed after the discovery of the antinomies consists in the development of the formalistic or metamathematical method of Hilbert.[12] The thought underlying this method may be condensed as follows: For every mathematical or logical theory, it must first be stated how the undefined fundamental concepts of the theory are symbolized, and how the propositions of the theory are constructed as sets of these fundamental symbols. For instance, there exists an axiomatic representation of Euclidean geometry in which the fundamental concepts are called points, lines and planes. One of the fundamental relations is that of "lying on." One of the rules by which geometric propositions are built up from these fundamental symbols is that, wherever the symbol for a point precedes the

[12] Easily comprehended expositions: Bernays, Blätter f. deutsche Philosophie, *4*, 326, (1936); Herbrand, Revue de metaphysique et de Morale *37*, 243 (1930); Hilbert "Die Grundlagen der Mathematik," Hamburger Mathematische Einzelschriften, vol. 5, Teubner, Leipzig (1928); von Neumann, Erkenntnis, *2*, 116 (1931).

words "lies on," the symbol for a line or a plane follows these words. An example of a logical theory, the axiomatics of the calculus of propositions, has already been given. The fundamental concepts are called "propositions" and denoted by the letters p, q, r, etc. The undefined fundamental relations are "not" and "implies." Secondly, the axioms of the theory must be formulated; that is, the sets of symbols corresponding to certain propositions are set down in advance, as initial formulae. And lastly, there must be given the formative rules whereby new sets of symbols may be deduced from sets which correspond to propositions of the theory. The propositions corresponding to these new sets are then taken into the theory. In the axiomatics of the calculus of propositions there were three such formulae and two such rules (see p. 305).

Thus the theory becomes a calculus, and the theory of this calculus is called the metatheory belonging to the original theory. This metatheory deals with the way in which the propositions of the original theory are connected, and how they may be derived from one another. It considers what propositions can be proved or refuted from the axioms, etc. The metatheory for the axiomatics of the calculus of propositions deals among other things with the question: What propositional formulae can be obtained by applying the two formative rules to the three axioms (initial formulae)? In the axiomatic Euclidean geometry, it is proved, for example, that the sum of the angles in any triangle is equal to 180°. That is, this theorem is deduced from the axioms by the help of the formative rules. On the other hand, metageometry investigates the question which of the Euclidean axioms are necessary for the deduction of this theorem. The proof that the axiom of parallels is independent of the other axioms is metageometrical.

The metatheory belonging to a given theory is above all expected to prove that the theory contains no contradiction—in other words that, in the theory in question, it is never possible to prove an assertion and its negative. By metageometrical considerations it is proved, that, if Euclidean geometry is free from contradiction, then non-Euclidean geometry is also free.

Moreover, it has been possible to show by metamathematical considerations that, if the theory of real numbers is free from contradiction, then Euclidean geometry is likewise. Now Hilbert hoped to prove by metalogic or metamathematics that a logic or mathematics founded on suitable axioms is also free from contradiction. In this way he hoped not only to get logic and mathematics over the crisis caused by the actual discovery of the antinomies, but also to make these subjects secure for all time by the valid proof that within them any sort of contradiction is impossible.

Before going into what has been brought to light by the use of metamathematics,[13] it is well to mention a few general results which are of interest in this connection. In geometry, beside Euclidean geometry, there have been deduced from other axioms other geometries which are quite different from one another and of which each is a system closed within itself. Similarly there have been constructed numerous logics which differ from one another; and each of these is a system closed within itself. Some examples are the so-called polyvalued logics which originated with Lukasiewisz and Post.[14] In the ordinary logic, all propositions are divided into two classes, the class of so-called true and the class of so-called false propositions, so that every proposition belongs to one and only one of the two classes, as expressed by the principle of the excluded middle or third. Likewise, there has now been developed a logic in which propositions are divided into three classes, and a principle of excluded fourth holds. The assumption that through one point there are many parallels to a given line leads to a system which is not only abstract but can even be illustrated by models; similarly, it is possible to get an illustration of the three-valued logic by dividing propositions into surely true, uncertain and surely false ones. In general, there is, for every natural number n, an n-valued logic which

[13] Metamathematics itself has recently been developed as a deductive theory in some important papers by Tarski, Monatashefte f. Mathematik u. Physik, *37*, 361 (1930); Fundamenta Mathematica *25*, 503 (1935) and *26*, 283 (1936).

[14] Post, American Journal of Mathematics *43*, 163 (1921); Lukasiewicz, Compt. Rend. Soc. d. Sciences et d. Lettres Warsaw, *23*, 51 (1930); Lukasiewicz and Tarski, ibid, p. 1. See also the older literature there quoted.

divides propositions into n classes and contains a principle of excluded n + 1. The ordinary logic with its dichotomy of propositions comes under this classification as a two-valued logic. To each one of these n-valued logics, there belongs a mathematics; but, of these, only the one associated with the two-valued logic has been closely studied. However, the mere existence of different logics and of different mathematics belonging to them is of interest, for until recently any such state of affairs was regarded as out of the question. It was, in fact, explicitly designated by Poincaré as impossible. Apart from this, these many-valued logics may well be related to the theory of probability.

Another variant from the classical system of logistics is Lewis's[15] theory of what he calls "strict implication." This term is applied to a relation between two propositions which comes nearer to the ordinary use of the word "implication" than the terminology of the calculus of propositions. It thus avoids certain paradoxical consequences of the latter, such as "a false proposition implies every proposition" or "a true proposition is implied by every proposition."

If the problem in hand is to prove by a metatheory that a certain theory contains no contradiction, it must above all be clear what means of proof are permitted for the metatheoretical considerations. If any geometrical or other special theory is to be proved free from contradiction, all of logic can, if necessary, be used as a metatheoretical tool, so that the upshot of the whole argument is a proof by logical inference that, in the system of propositions of the theory in question, there is no contradiction—no assertion appears with its negative. What is meant, however, by a proof that logic itself and the mathematics involved in general logic are free from contradiction? Here, in a proof of self-consistency, not all of logical inference can be used in the metatheory, otherwise the very system whose self-consistency was to be proved would be used as a means of proof. The object is naturally to prove self-consistency for as large a part as possible of logic and mathematics by the use of a minimum of metalogical methods of proof, and eventually in this way to cover all parts of

[15] Cf. Lewis and Langford's book quoted in (1).

the subject. For example, Herbrand succeeded in proving the self-consistency of the calculus of propositions and a part of the theory of natural numbers (containing the principle of the excluded middle) by the use of complete induction, but without the use of the principle of the excluded middle.

The program was to push forward to a proof of self-consistency for all of mathematics. Considering that a portion of mathematics is to be used in this proof, the fundamental problem may be formulated as follows: To prove the self-consistency of all logic and mathematics by the use of a part of logic and mathematics. Such was the state of science until a short while ago, in fact until 1930, when Gödel succeeded in making a completely unexpected and most significant discovery. He solved the fundamental problem, but in a negative sense, for he proved metalogically by the use of only the theory of natural numbers that the self-consistency of mathematics and logic cannot be proved by a part of mathematics and logic.[16] And this conclusion is not the result of a flaw in the system of logical axioms, after the correction of which logic might be proved self-consistent, for Gödel proved the general theorem: Any formal theory which contains the theory of the natural numbers cannot be proved self-consistent by means of principles which can possibly be expressed within the theory in question. No matter how the system of logic is modified, provided it remains inclusive enough to serve as a foundation for the theory of the natural numbers, it still cannot be proved self-consistent by methods which can be expressed within the system. If there are added certain other metalogical methods of proof, which cannot be formulated within the logical-mathematical theory to be proved self-consistent, then the self-consistency of the theory under consideration can be proved; but then the methods of proof are, at least in certain respects, more inclusive than the theory whose self-consistency was to be demonstrated. For example, the theory of the natural numbers can be proved self-consistent, if operations with any class of natural numbers are taken for granted—that is, essentially, if operations with real numbers are admitted. What has turned out to be impossible

[16] Cf. Gödel, Monatshefte f. Mathematik u. Physik, *38*, 173 (1930).

is to prove with a portion of mathematics the self-consistency of a more inclusive portion of mathematics, provided the latter contains the theory of the natural numbers. What can be proved self-consistent by a portion of mathematics, is in general only a narrower or overlapping portion of mathematics. In other words, for the proof of the self-consistency of a portion of mathematics, a more inclusive or an overlapping portion of mathematics is necessary.

This result is so fundamental that I should not be surprised if there were shortly to appear philosophically minded non-mathematicians who will say that they never had expected anything else. For it should be clear to a philosopher that no theory, the structure of which contains no superfluous parts, can be founded on one of its parts, etc. etc. But when such general principles are applied to metalogical problems, they turn out to be not only not self-evident but false. For example (as already stated) it can be proved from the axioms of the theory of real numbers, that, from the self-consistency of this theory, there follows the self-consistency of n-dimensional Euclidean and non-Euclidean geometry, although n-dimensional geometry includes the whole theory of the real numbers as a part, in fact as a one-dimensional special case. On the other hand, Gödel's investigation shows, for example, that it cannot be proved from the axioms of the theory of natural numbers (first systematized by Peano) that the self-consistency of the theory of real numbers follows from the self-consistency of the theory of natural numbers. Similarly, it cannot be proved by certain parts of the theory of natural numbers that the self-consistency of the whole theory follows from the self-consistency of these parts. This contrast shows clearly that Gödel's discovery is not a self-evident remark based on general principles, but a fundamental mathematical theorem which needs proof and can be proved.

Quite recently another important result concerning the self-consistency of mathematics has been obtained by Gentzen.[17] He uses in his metamathematics a part of the theory of natural

[17] Gentzen, Mathematische Annalen, *112*, 493 (1936). Transfinite methods in the theory of proofs are also used by Church.

numbers together with certain transfinite methods. The part of arithmetic used includes complete induction but not what Gentzen calls questionable portions of arithmetic. By these means he proves that the whole of arithmetic including the questionable portions just referred to is free from contradiction. In this case a system of axioms leads to a proof of self-consistency for an overlapping system of axioms.

A second part of Gödel's discoveries is another good illustration of the force of metamathematical methods. In order to explain this it is necessary to digress. It was one of Euler's greatest discoveries that theorems concerning the natural numbers can also be proved by so-called transcendental methods—that is by the help of considerations which go beyond natural numbers and the principle of complete induction, since they make use of the concepts limit and continuity as well as of operations with all real numbers and functions. For example, the theorem (discovered by Fermat and proved by elementary means) that every prime number of the form $4n + 1$ can be expressed, and in one way only, as the sum of the squares of two natural numbers has also been proved by transcendental means. No matter how much Euler's discovery was admired (and a branch of mathematics, the so-called analytical theory of numbers has developed out of it), there still persisted the faith that all theorems about natural numbers could be proved by elementary means. For even when elementary theorems were formulated for which only proofs by transcendental means were found, this fact was ascribed to the circumstance that elementary proofs for these theorems had not yet been discovered. But Gödel has proved metamathematically that there are surely theorems and problems about the natural numbers which cannot be proved or solved by elementary means; for their proof or solution transcendental methods must be employed. Among these unsolvable problems are some concerning certain Diophantine equations,[18] such as: Does the equation $P(x_1, x_2, \ldots x) = 0$ where P is a polynomial with integer coefficients admit of integer solutions? Fermat's hypoth-

[18] Cf. Gödel, Ergebnisse eines mathematischen Kolloquiums, *7*, 23 (1936). Cf. also Church, American Journal of Mathematics, *58*, 345 (1936).

esis (previously mentioned) is nothing but the question whether the Diophantine equation $P(x, y, z) = 0$ has integer solutions when P is the polynomial $x^n + y^n - z^n$ and n is greater than 2. An example of a proposition which can neither be proved nor refuted by elementary means is the following: The Diophantine equation whose insolubility by elementary means was proved by Gödel admits an integer solution. This proposition is of the same type as Fermat's hypothesis. The question of unsolvable problems is thus today in the same state as the theory of transcendental numbers was in the middle of the last century. At that time Liouville had constructed extensive classes of transcendental numbers, but none of the numbers which had been dealt with in other parts of mathematics were known to be transcendental. Only some decades later was it proved that two of the best known numbers (e and π) are actually transcendental. For logic and the theory of knowledge it is of course unimportant that, so far, no classical problem in mathematics has been turned out to be insoluble. The important fact is that there are certainly problems of the same type as those now under discussion which can be formulated and proved to be without solution.

Again this result does not depend on the insufficiency of the special assumptions about the natural numbers. On the contrary, there is a proven general theorem[19] that in every formal theory which includes the whole theory of the natural numbers there occur problems which cannot be solved within the theory in question. Just as there are propositions about natural numbers which can be proved only by methods taken from the theory of real numbers, so there are propositions about real numbers which can be proved only by methods taken from the theory of sets of real numbers. And there are problems about sets of real numbers which can be solved only by assumptions about sets of higher power. In fact, in every formal theory including the whole theory of the natural numbers, there are statements about natu ral numbers which can not be proved within the theory in ques-

[19] Cf. Gödels paper quoted in (16). Skolem (Norsk. Matem. Forenings Skrifter, p. 73 (1933)) proved that it is impossible to characterize the sequence of natural numbers by a finite number of axioms if the law of the excluded middle is one of them.

tion. In other words, a universal logic (such as Leibniz dreamed of) which, proceeding from certain principles, makes possible the decision of all conceivable questions, cannot exist.

Such is the state of formalism today. What has it accomplished? Evidently a proof of self-consistency for all mathematics and a demonstration of the solubility of all its problems (the original objectives for the sake of which Hilbert founded and developed metamathematics) have not been and, in a certain sense, cannot be attained. Does this mean that a third crisis in logic, a crisis in metamathematics, has occurred? In my opinion, Gödel's results do not show that the formalistic metamathematical method has failed. Only a collateral philosophical interpretation has proved to be one sided and partly wrong. Hilbert's great and undying merit in creating metamathematics lies in the fact that he introduced a precise method for studying and discussing the mutual relations of different mathematical propositions, different methods of logical inference, etc. Truly, the application of this method, particularly in Gödel's hands, has led partly to the destruction of illusions, but it must not be overlooked that the formalistic method alone has made possible an attack in rigorous fashion on problems like self-consistency and solubility. It has replaced vague philosophic talk on such subjects by precise demonstrations. This vigorous research will certainly continue.

Formalism and metamathematics have made it possible to deal generally, systematically and accurately with the question which in my opinion is of supreme interest for mathematicians and logicians: What can be inferred from given hypotheses (precisely formulated systems of propositions) by given methods of inference (precisely formulated systems of rules for the transformation of propositions)? Under this heading come also the intuitionist systems at any given moment, although some intuitionists are not fond of formulating precisely what hypotheses and methods of inference they do at the moment actually use. It can not be repeated too often that various intuitionist systems have been constructed, and that still other and more restricted ones might be. Almost every one of these theories so far invented has

claimed to be the one and only intuitive mathematics and has ruled out more far-reaching systems. But, in my opinion, this attitude is a philosophical interpretation unessential for mathematics, since it plays no rôle at all in the system itself. I should treat this interpretation as a physiologist might treat the different diets preached by various philosophical schools. He would be interested; he would discuss and study the consequences of each one of these diets for bodily and mental well-being; he would compare the consequences of different diets, etc. But he would leave to the individual the choice of which diet to adopt. In mathematics the essential fact is that various systems of hypotheses and various systems of rules of inference (rules for the transformation of hypotheses) may be adopted. Anyone is free to choose as he pleases.[20] Precisely what is interesting is to observe the development (that is the investigation of the consequences) of different systems, and to compare them one with another.

In 1932 I tried to sum up this view in the following words.[21] "What interests the mathematician and all that he does is to deduce propositions by certain methods (which must be formulated) from certain initial propositions (which must also be formulated). There is a variety of ways to choose both the methods and the initial propositions. And to my mind all that mathematics and logic can say about this mathematical activity (which neither needs justification nor can be justified) lies in the statement of these simple facts. Which initial propositions and methods of inference the mathematician and logician choose, and what is the relation of these propositions and methods to so-called reality and to an inner feeling of conviction (Evidenzerlebnis)—such questions belong to other and less exact sciences."

[20] For this reason, the frequent statement that mathematics is a great tautology or system of tautologies does not seem to me an adequate description of the situation. The concept of tautology has so far been defined only within the calculus of propositions. This concept might, it is true, be defined by methods lying outside this calculus, but it might be defined in different ways, whereas the words "Mathematics is a system of tautologies," suggest a reference to an absolute logic.

[21] "Die neue Logik." Krise und Neuaufbau in den exakten Wissenschaften. Deuticke, Vienna, 1933.

What I have said so far must not be misunderstood to indicate that I consider mathematics meaningless or merely a game. I do believe, it is true, that mathematics has important esthetic qualities. It may be left to the psychologist to describe them fully. Partly they consist in an enjoyment of a particular kind of mental activity; partly they resemble the receptive appreciation of music. And no one reproaches music with the fact that its human importance is restricted to its esthetic aspects. Unfortunately the appreciation of mathematics is much rarer than that of music; however this seems to be essentially only an effect of bad teaching.

But no matter how many-sided these entertaining aspects of mathematics may be and indeed are, the importance of mathematics is not restricted to them. This fact is most clearly evidenced by geometry, the axioms of which are related to empirical facts about what is called space. A hundred years ago, what could have seemed to the superficial observer more like a mere game than deductions based on the assumption that in a plane there is more than one parallel to a given straight line through a given point? And yet it is known today that the paths of light rays and of mass points under the influence of gravitation behave like the straight lines in these speculative systems which seem at first to be mere games estranged from all reality. Moreover, these curious deductions have drawn attention to a wealth of further relations between entities which can be experienced and observed, while at the same time they have furnished means for a quantitative check on the assumptions involved. Clearly the views on mathematics here outlined by no means imply that geometry is a mere game or that its propositions are unimportant or meaningless. They only express the belief that the specific function of the geometer is not to deal with these applications, important as they are. For practical and historical reasons, applications are better left to the physicist. The geometer himself has work enough to do in proving new theorems, in studying the mutual relations of the theorems in any single system, in comparing different systems and in creating new ones. Theorems in other parts of mathematics are related to other

parts of physics, to the theory of heredity, to the theory of human economic behavior, to ethics, etc. The concept of mathematics here described denies neither the possibility nor the great importance of mathematical applications.

There are also relations between certain hypotheses and modes of inference on the one hand and certain rather vague psychological phenomena like inner conviction or intuition on the other. It is important though usually overlooked that such phenomena change with time, and depend upon history and practice. Or is it to be believed that the methods of inference and construction which are self-evident to an intuitionist in 1930 are identical with the methods of inference and construction which were self-evident to Pythagoras, to Archimedes or to Euler, or which will be self-evident to a mathematician in 2930? Again, the relation between certain parts of mathematics and the feeling of inner conviction is important both from an historical and from a psychological point of view. I only question whether it be practical to deal with such problems within mathematics itself. But if they are so dealt with, I would insist that the mathematician sharply separate them from the mathematical theories in which they play no rôle.

But mathematics (and particularly mathematics as described above) besides its esthetic qualities and the importance of its relations and applications to many sciences has another highly important function. It clarifies discussion on any subject. The theme may be philosophical problems, methodological theories or sociological questions—although this last aspect of mathematics has not yet been extensively developed.[22] Yet this clarifying function may turn out to be of considerable importance for mankind. Mathematics might and perhaps will be the source of a new insight into human intellectual affairs, as well as into practical affairs, so far as they involve intellectual elements.

The importance of mathematics beyond its esthetic qualities is in no wise denied by the assertion that mathematics transforms

[22] An attempt in this direction is my paper, "Einige neuere Fortschritte in der exakten Behandlung sozialwissenschaftlicher Probleme." Neuere Fortschritte in den exakten Wissenschaften, Deuticke, Vienna, 1936.

precisely formulated hypotheses according to precisely formulated methods. Restriction of all discussion on the "foundations" of mathematics by this simple statement serves to emphasize the freedom of choice in respect to both hypotheses and methods. Yet this point of view, although it has often been urged since 1927, has so far had no considerable influence on discussion. Still I hope it will have in the future. Intuitionists in particular have paid no attention to the criticism formulated under three heads on page 322. Only quite recently I have had the pleasure of seeing Carnap state his agreement with this point of view. Freedom of choice in regard to hypotheses and methods of inference he has called the "principle of logical tolerance," and in the introduction of his book "Logische Syntax der Sprache" he describes the effect of this principle (which lies close to the center of his whole theory) in overcoming historical barriers and opening up a world of free possibilities.[23]

What then are the conclusions as to the nature of mathematics? In recent years particularly, the profoundest parts of this science have developed in such a way as to inspire with admiration the very persons who best understand fundamental logical relations. On the other hand it has been impossible to make the subject secure against the occurrence of contradictions. I would say: Mathematicians are like men who build houses. These are not only pleasant to live in; they enable their inhabitants to do many

[23] After introducing the term "principle of logical tolerance," Carnap (logische Syntax der Sprache," Springer, Vienna (1934)) suggests that, in first explicitly formulating this principle, I was expressing the attitude of most mathematicians. I should be glad if Carnap were right, but since the prominent mathematicians (Poincaré, the Paris school at the beginning of the century, Hilbert, Weyl, Brouwer) who have dealt with the foundations of mathematics have explicitly expressed opinions which, divergent as they are from one another, are all diametrically opposed to the above mentioned principle, I am afraid that I must bear the responsibility alone.

I take this opportunity to emphasize that I do not agree with the pronouncements on metaphysics which come from the group of which Carnap is a member. It is true that an expert logician can easily find logical errors, and very elementary errors at that, in many metaphysical theories, just as he can in many economic and sociological ones. Such errors are, however, objections only to special existant systems. If all metaphysics is rejected because its statements cannot be tested, then, I am afraid, very extensive parts of mathematics must likewise be discarded. Thus one arrives at arbitrary statements which resemble some of the critiques of classic mathematics discussed in this paper.

things which a cave-dweller could never accomplish. Mathematicians are like men who build, although they are not sure that an earthquake will not destroy their buildings. If an earthquake should destroy their work, new constructions, if possible such as promise to be more resistant to earthquakes, will be erected. But men will never permanently decide to give up the building of houses with all their conveniences because of possible earthquakes—the more so because absolute security against the effects of earthquakes is not to be obtained even by the inconvenient habit of cave-dwelling. Mathematics seems to me to be in the same state. Not only is it a pleasure in itself, but it serves many other important purposes as well. Its various edifices are not secure against the earthquake of a contradiction. But men will not cease on that account to elaborate its constructions and to erect new ones.

University of Chicago.

Critical Points in Modern Physical Theory

BY

HENRY MARGENAU

RECENT discussions in the physical literature,[1] designed to clarify the logical position of modern physical theory, have brought to light an amazing divergence of fundamental attitudes which may well bewilder the careful student of physics as well as philosophy. Quantum mechanics, representing an abstract formalism, should be capable of having its logical structure analyzed with great precision like any other mathematical discipline. Its consequences in all problems to which its method can be applied are so unambiguous, consistent, and successful in predicting physical experience as to disperse immediately all thoughts of possible discrepancies in its fundamental texture. Yet it must be said that even the founders of quantum theory are not in harmony in their various expositions of the bases of that theory. However, while this situation seems disquieting on the face of it, there is no cause for serious brow raising, for it is a fact that there exists agreement with regard to the central axioms of the theory, and that the ambiguities affect only their philosophical interpretation, a field in which differences of opinion may at present be honestly entertained.

In the present article we desire to draw attention to several ways in which these axioms can be and have been interpreted. Without attempting finality we shall investigate the consequences

[1] See bibliography at the end of this paper.

of different views and separate clearly their incompatible features, appraising now and then the various possibilities as to their reasonableness and their degrees of popularity among scientists. In the course of this endeavor there will arise occasional suggestions dealing with wide-spread terminology, whose aim will be to guard against confusion of language and to eliminate certain annoying contradictions the origin of which is purely verbal. There will be an analysis and proposed definitions of such important terms as state and measurement, for it is believed that they have not yet acquired universal meaning and are for that reason the chief source of confusion in discourses on the foundation of modern physics. We begin by outlining those axioms of the new quantum mechanics on which the description of nature essentially depends.

I. THE AXIOMS OF QUANTUM MECHANICS[2]

There are two ways in which the basic postulates of any theory may be presented: the inductive and the deductive. Textbooks on quantum mechanics generally use the former. They start by giving an account of the failures of classical physical description, then proceed to involve the reader in the intracacies of measuring devices, tell him with enormous emphasis that there is absolutely no hope of ever performing a measurement which achieves simultaneous accuracy for complementary (conjugate) physical quantities, dazzle him with Heisenberg's uncertainty relations and finally introduce him to the use of matrices and general operators. This process is awe-inspiring and pedagogically effective with minds that wish to learn rapidly. But it leaves unsatisfied the student who is interested in fundamental questions of meaning, for the steps lack logical compulsion and uniqueness. In the first place, no inductive proof, based on experiments, can ever be given for the impossibility of increasing the accuracy with which canonically conjugate observables can be measured. Secondly, the complete meaning of indeterminacy is difficult to convey by examining individual measurements alone,

[2] Only a general discussion is here intended. For a complete statement see P. A. M. Dirac, Principles of Quantum Mechanics, or Lindsay and Margenau, Foundations of Physics, p. 401.

and finally, the particular choice of operators which is made in quantum mechanics does not follow uniquely from the uncertainty relations. It may be that the general use which this particular mode of presentation enjoys everywhere is partly responsible for the uncritical attitude toward basic matters which many working physicists have so long displayed.

The second, the deductive method, does not pretend to *prove* anything new—a semblance which often attends the former procedure—but resigns itself to an enumeration of a new set of axioms in terms of which we agree tentatively to describe our physical experience. The analysis of the situation thus created, once the postulates are stated, is a matter of complete logical and mathematical rigor, and the consequences are capable of experimental test. Success in fitting data strengthens the belief that the initial axioms are a suitable starting point in physical investigations, though it cannot convey the conviction that these axioms are unique, for it might well be that another set were equally successful. This question of uniqueness will probably never be settled, and it is a fortunate circumstance that we have no means for doing so, a circumstance which prevents our theories from freezing into rigid patterns, which keeps our inspiration and our hopes alive. But strongest confidence in the essential "*correctness*" of the axioms, which have proven their worth in empirical predictions, comes when we begin to interpret them and find that they differ from the postulates of classical physics precisely at those points where critical analysis has already shown the latter to be defective. In this manner the new theory acquires a degree of certainty which is the maximum attainable. This form of presentation, which exposes more truly the logical status of quantum mechanics and provides a ready answer to the question of its ultimate correctness, will here be chosen although it does not possess the obvious pedagogical advantages of the former method.

Physical analysis deals with the behavior, imagined or observed but always in principle *observable*, of physical systems.[3] Quantum

[3] Physical systems are in general constructs in the sense of a previous communication (Journal of Phil. of Science, 2, 48 (1935); 2, 164 (1935)). We shall here assume the terms to be so understood.

mechanics, in particular, has to do with systems observable only by indirect means, principally because of their smallness. To analyze their behavior the physicist invents *states*. Now in common language a state carries with it the connotation of some mysterious objective reality, whatever that word may mean; it implies something entirely independent of our description, something to which our description must conform. As a matter of physical convention, however, a state is deprived of this exalted independence and is entirely at our mercy. We determine how we are going to describe states, and the agreement with respect to this matter constitutes the first axiom of any theory. In classical mechanics states were defined, or specified, by means of coordinates and momenta of mass points; in thermodynamics an altered convention is being used: by state is meant a coordination of two thermodynamic variables, such as pressure and volume of a system. In quantum mechanics, finally, we agree to mean by state, or the representative of a state, a certain function of a suitable number of variables.

The distinction between state and representative of a state has here been made merely as a concession to ultra-meticulous readers. Such a difference exists only on the scale of ordinary experiences where an intuitive conception of the coordinates and momenta of a body, as a bullet in flight, is obtainable. Here one can associate an unambiguous mental picture with the state which has as its representative a certain group of positions and momenta. On the atomic scale, where no such picture can be obtained except by intrinsically unverifiable extrapolation of large scale experience, the dangers of which are now quite apparent, it seems in order to drop the distinction and refer to the "representative of a state," which alone is available, briefly as the state.

But the introduction of a state function is exceedingly abstract and is not at all linked with physical experience. What is the operational meaning of this procedure? This leads to an important point. It is true that classical physics did formulate its states in terms of measurable quantities, as well it might, for it operated on a scale of visual perception. But this should be regarded as a coincidence rather than a logical necessity: *there is*

no consideration which compels that states be directly measurable. All properties, to be sure, with which physical systems are endowed, must be measurable. States are invented in order to enable statements regarding measurable physical properties to be made, which is by no means synonymous with saying that states must be measurable. In quantum mechanics it happens indeed that states become intermediators between abstract constructs called systems and their measurable properties.

The invention of states which, while they are themselves not observable, would also fail to provide a *definite connection* with experience, must be considered as futile. But quantum mechanics is not in such sterile condition. By means of a second axiom a very precise link is made between a system characterized by a given state and its observable properties. The connection is not of the usual reversible type but involves probability aggregates. The axiom posits that when a state function is given, the exact probability distribution with regard to every observable quantity can be computed. The detailed manner in which this is to be done is not relevant to the main theme of this discussion. We have thus established a one-one correspondence, not between states and observed properties, but between states and probability distributions of observed properties. If causality were to be defined as residing in a unique transition between state and observation, then the description to which this axiom gives rise would have to be termed non-causal. Practically speaking, when the state of a system is known, the probability that a given value of the momentum shall be observed can be predicted, but the outcome of a given momentum measurement is in general obscure. One of the notable consequences of this axiom is Heisenberg's famous principle of related indeterminacies. It states that the standard deviation of the probability distribution with respect to the momentum of a system multiplied by the standard deviation of the probability distribution with respect to its measured position is never smaller than a certain universal constant (Planck's h divided by 4π; momentum and position may here, of course, be replaced by any other two so-called canonically conjugate quantities). The reader who is unfamiliar with the statis-

tical term "standard deviation" may in this connection think qualitatively of the spread of the measurements.

We have already indicated that on the basis of the present formalism a single measurement can never suffice to determine a state. This leads at once to the important question: Can the state function be inferred at all, even from a number of observations sufficient to determine all probabilities with respect to a given observable (measurable quantity)? The answer is not an unqualified yes. Let us take a simple example. We wish to determine by observation the state function of an electron about which nothing is known at present. For this purpose we might decide to measure its energy. According to our preceding considerations, we must not expect to find the same energy value every time the measurement is repeated on the system in the same state (supposing here that we have means of reproducing the state; cf. below). Hence we are forced to determine how many measurements yield the value E_1, how many yield the value E_2 and so forth. If we perform altogether 1000 measurements, 100 of which have given the value E_1, we say that the probability of that occurrence is 1/10 etc. Finally we shall be in possession of a sequence of numbers $p_1, p_2, \ldots$, the probabilities of the individual measured values, the totality of which constitutes the probability distribution. The point is that from this knowledge the state function can *not* be constructed. It involves, not the p-values, but numbers characterized merely by the requirement that the squares of their absolute values shall equal the p's, and this requirement is not sufficient to specify these numbers completely. Thus it would be impossible in our example to predict the probable distribution of momentum measurements on the basis of the results obtained with regard to the energy.[4] This proposition is proved in detail in the appendix (A1).

The situation here described is indeed disturbing; it would seem to require that the description of states adopted in quantum mechanics is forever aloof from physical experience, that states can never be known completely through observation. But this

[4] Excluding, of course, those special cases, like the free electron, where energy and momentum operators commute.

is not the case, at least not in principle. Suppose, for instance, that our energy measurements had all yielded the same value, which is entirely possible. Then we should have known the state function with sufficient completeness to predict the probability distribution with respect to every other type of measurement. It is true in general that if a given kind of observation does not spread, the statistical distribution governing the observations on all other quantities can be inferred, and this is the only condition under which such an inference is possible. We are now in possession of the clue which can ideally lead to an experimental determination of any state function: If the particular kind of observation which we happen to perform is seen after a few trials to spread, we proceed to measure other quantities until one is found with respect to which the state is "pure," i.e., which yields the same value in all trials.

But now we come to the troublesome question: does such a quantity exist? Nobody knows. If this answer implies, in good positivistic fashion, that such a quantity does not exist, then the matter is settled for the time being: a state function can not in general be found, or constructed, on the basis of measurements. Such a position, by the way, is entirely tenable and does not reflect in the least upon the usefulness or legitimacy of quantum mechanics as a theory. But let us not dispense with this matter too hastily. There are, after all, those who hold that the number π existed before it was discovered.

The question as to the existence of a quantity whose measurements will not spread for an arbitrary state function resolves itself on closer analysis into two; first, the mathematical one: does an arbitrary function of a fixed number of variables, which satisfies certain (boundary) conditions, possess an operator with respect to which it is an eigenfunction? second, is it possible to associate with every operator an operation which could be regarded as a measurement of a physical quantity? It is clear that the number of possible mathematical operators as well as the number of physical operations is infinite, so that there is an unlimited choice. While it is at present a pious hope that the difficult program of correlating functions, operators, and physical

operations in the unique manner here required will ever be carried out, there is at least no basic obstacle in the way of its execution. The fact is of course that the interest of physicists has been limited to relatively few physical quantities which can be represented by about half a dozen different operators, and that research has been remarkably successful despite this limitation. On the whole it seems not unreasonable to answer the question at the beginning of this paragraph in the affirmative, and thereby to provide an ideal basis for linking states with observations.

The first axiom had to do with the definition of states; the second dealt with the relation between states and experience and involved several matters which can easily give rise to misunderstanding. We now take up the thread of the original discourse and introduce the third and final axiom. It is clear and concise and requires very little comment. It amounts to a recipe for determining what possible values any given physical quantity can assume when a measurement is made. In barring much, it imposes a most daring restriction upon experience and exposes itself to exceedingly severe tests. Suffice it to say that it has successfully survived many tests and may be said to be in good standing. The structure of this axiom is mathematical; it states that only eigenvalues of operators associated with physical observables can appear as the results of measurements. But a complete technical understanding of these matters is hardly necessary for the subsequent discussion.

II. CLOSER ANALYSIS OF THE MEANING OF STATES

We now leave aside the factual content of the postulates and propose to view them—as far as we are able to do so—from the vantage point of the philosopher. At the same time we are entering upon controversial ground. States were said to be (represented by) certain functions having an indirect but definitely specifiable connection with possible experiences. They served to describe the behavior of physical systems. Let us be more precise and examine whether these states describe *physical systems* or merely *our knowledge of physical systems*. The answers to several controversial questions depend on the stand which is

taken on this particular issue as will appear later, and it is very surprising that the importance of a clear-cut decision in this matter has rarely been recognized even by the most authoritative writers. It is difficult to find an article in which the author, though evidently speaking from one point of view, does not somewhere permit himself at least a verbal lapse toward the other. A middle stand (or shall we say seat, for this is by far the most comfortable position) is taken by many who pronounce the question as meaningless and consequently recognize no distinction. This possibility must also be investigated.

The contrast between physical systems and our knowledge of physical systems is not as deep as the epistomological difference between realism and subjective idealism, and the philosopher who hopes for a decision between these issues from any physical theory will be disappointed. No physical theory is tied to any such metaphysical basis. Classical physics made sense to Newton as it did to Berkeley, the only difference being that Newton thought of physical systems as objective entities transcending consciousness, while Berkeley regarded them as existing within consciousness, but nevertheless "existing." So in quantum mechanics: whether we place physical systems outside ourselves and contrast them with conscious knowledge of them in a fundamental epistemological sense, or whether we recognize, within consciousness, a difference between knowable systems and knowledge of them, the systems themselves being merely constructs—the choice does not matter as long as we maintain an intelligible distinction between the concept of system and that of knowledge about the system. This distinction, it seems, is so thoroughly ingrained in our thinking, so vividly alive in our intuition that it would be difficult to eradicate even if there were logical or metaphysical grounds for doing so. Or must we, after all, yield to superior logic and admit that the distinction fails? If the electron has no transcendental "reality" but is only a construct with which we operate, does it not, as a concept, become identical with our knowledge, i.e., awareness of it? It seems that, unless we are willing also to confuse the existence of the solution of an equation with our knowledge of that solution, the answer must be no. But

there are in many cases definite rules for telling whether a solution exists, while to know the solution would be an altogether different matter. Logically, we feel that the distinction must be maintained, and this is fortunate, for otherwise logic would painfully contradict our intuition.

Having cleared away the metaphysical underbrush we may now proceed with greater ease. Does the quantum mechanical state function refer to the system, or does it describe our knowledge? Planck, who according to his recent writings professes a very wholesome realism, would seem to favor the first alternative. The spirit of Dirac's writings, although they contain no specific pronouncements, indicates that his states refer to systems apart from our awareness. Schrödinger, on the other hand, speaks occasionally of the state function as the "maximal sum of knowledge," indicating perhaps a preference for the second view. Bohr and Heisenberg mention frequently the conflict between observer and observed object with their unavoidable interaction and thereby presumably express themselves in favor of an objective world, the properties of which are to be described according to the first view. But this last diagnosis may be in error.

Let it be said at once that neither position can be wrong in the sense that it leads to erroneous consequences if carried through correctly. We mean here consequences of a positively verifiable character. The logical implications are in some respects very different, and this is particularly true about the meaning of probability in the two cases. But before examining the necessities of interpretation enforced by each of the two points of view, it may be well to pause for a moment and to see if one of them is not more plausible, or more in line with other accepted doctrines than the other.

In common language, which after the foregoing orienting excursions may perhaps be used without fear of misinterpretation, the distinction in question is simply that between physical objects and the observer's knowledge about physical objects. In classical mechanics the distinction was extremely sharp, and it was supposed that the two, besides being generically different as logical entities, were independent even in a physical sense. States

clearly referred to the objects themselves, positions and momenta were their direct properties. The objectivity of states had its culminating expression in the equations of motion, which were understood to imply actual space-time propagation of systems. This propagation took place according to rigid laws independently of the observer's knowledge.

Such an extreme view regarding the independent objectivity of physical states cannot be carried over into quantum mechanics; there must at least be a shift of emphasis. One of the great discoveries at the beginning of the present era in physics was the recognition that objects and knowledge are related, because the classical notion of ideally unlimited accuracy of experimental devices failed and made classical knowledge intrinsically impossible. Hence if the view of objectivity be adopted, its classical meaning must be modified to this extent. It goes without saying that states must also be stripped of all impediments pertaining to sensual perception, and thought of entirely in abstracto. Psychological objections to this procedure, frequently raised by members of the older school, are of no particular moment in this connection, for it is not a matter of convenience, but one of logic which concerns us here.

The objective view of quantum mechanical states is enormously strengthened by the fact that states develop in time according to a definite differential equation (Schrödinger's) which has a form not unlike the equations of motion in classical physics. It is true that the simple interpretation of spatio-temporal propagation presents its difficulties or is at any rate no longer intuitively direct, but the fact remains that there are determinate changes in time which are difficult to correlate with simultaneous changes in the observer's knowledge because they seem enforced dynamically and not psychologically.

There are further points which may be cited as evidence for the plausibility of the objective view. We have seen (third axiom) that quantum mechanics makes very drastic positive predictions about possible experience, predictions which do not resemble laws of thought and give an impression of utter independence of human knowledge. To be sure this axiom has little to do with the

definition of states, but it does indicate that part at least of the new formalism concerns itself with matters that are thoroughly factual in a sense distinct from mere knowledge. And if this is true there seems to be little motive for insisting that another concept, that of state, should deal not with factual objects but with our modes of awareness.

It was pointed out that classical mechanics in all its structure suggests the objective view. Now it is possible to show that, if applied to large scale bodies, quantum mechanics leads to the same physical consequences as does the classical theory. The latter may be said to be the analytical continuation of the former in the field of ordinary experience. This is felt to be a very happy circumstance which bespeaks the unity of physics and encourages the hope that some day a universal theory may be achieved, a hope which at present animates the researches of Einstein. In view of the analytical smoothness with which the two disciplines join it might seem unwise to maintain in one a fundamental attitude which has no place in the other and which would constitute a conceptual break at the passage from one to the other. This argument, too, would suggest an adoption of the objective view. But it should be observed that the points here presented can aim at no more than plausibility, and do not render the opposite view basically untenable.

The opposite view, to be called for brevity the subjective view, is the one which holds that state functions describe our knowledge of physical systems. It is psychologically motivated by the significant observation that the independence of classical states is a fallacy. It springs from the desire of making a clean break with erroneous notions and therefore emphasizes an opposite extreme. Let us first see what can possibly be meant by it. Obviously it can not mean that the state function describes the observer's awareness, his momentary state of mind, and the changes in it which occur in time. It is clear that all this depends on factors far removed from the field of physics. Quantum mechanics certainly does not have the aim of converting physics into a highly expressionistic type of psychology. Although proponents of this view often employ ambiguous language, what they wish their

states to represent is not actual knowledge but potential knowledge. The state function is to be regarded as a convenient carrier, a symbol for the sum total of all that knowledge which the speculative observer can possibly accumulate, at any instant, with the use of all his resources. It may coincide with that knowledge at certain moments of very acute awareness, that is, when the investigator has made all possible measurements and all calculations pertaining thereto. No one can deny that this view, even in its extreme, has in it a trace of objectivism because of the admission that states refer to potential and not to actual knowledge, and this is a feature which makes it slightly inconvenient to have to defend the subjective position; it demands constant vigilance lest an inadvertant lapse should contradict the initial tenet.

The chief virtue of the standpoint under discussion is that it provides complete safety against positivistic attacks. Its tenant can justly pride himself in being under no obligation to transcendental agencies for transmitting his knowledge, for he deals with nothing but knowledge which he may believe if he wishes to be generated within his mind. He may wonder perhaps why the laws which govern the evolution of potential knowledge operate with almost dynamic regularity, but this is after all no greater a miracle than the existence of rigid laws in an objective world.

The author has not succeeded in bringing to light any further merits of the latter, subjective view. While admitting its possibility on logical grounds, he wishes to record his preference for the former. This preference is partly based on reasons so far presented, partly on considerations of the meaning of probability peculiar to the two attitudes which will now be discussed.

The contrast between objectivity and subjectivity of states has an interesting analogue in the theory of probability,[5] which, as the reader very likely recalls, can be formulated either as an *empirical frequency theory* or as a *subjective* ("a priori") *discipline*.

In the former case, probability is defined as the limit, with increasing number of trials, of the relative frequencies of an occurrence.[6] Probability can be ascertained only empirically, by con-

[5] The philosophical consequences of the quantum mechanical probability formulation have been stated clearly by F. S. C. Northrop, Philosophy of Science, 1936, pp. 215 ff.

[6] For a detailed discussion see Lindsay and Margenau, p. 161, Wiley and Sons, 1936.

tinued observations. There are difficulties inherent in this conception, especially mathematical ones connected with the existence of the limit, but it seems that they can be removed. The conception has, however, the great advantage of being applicable to *all scientific problems* which call for the use of probabilities. Notable also is the circumstance that it permits probabilities to be *measured* numerically to any desired approximation in accordance with their definition. It is not possible, on this view, to predict probabilitics by *a priori* considerations peculiar to the probability calculus, although this may in many cases be done tentatively by introducing special physical theories. (Thus the probabilities for tossing heads or tails can be ascertained directly only by making a large number of trials. Yet there is nothing to prevent us from considering the coin as a physical body, then making the assumption of symmetrical mass distribution and supposing a plausible distribution of initial conditions as well as a sufficient variety of them—a basis on which the probabilities may be computed by means of a physical theory.)

But there are cases to which the empirical probability conception can not be applied. They constitute the class of all propositions about *single events*, such as the statement: it will probably rain tomorrow. Such statements have the peculiarity that no unambiguous measure can be attached to the probability involved, except that, on retrospect, the probability is one if the event has happened, zero if it has not taken place. These cases are covered by what is usually termed the *subjective* or *a priori* probability formulation mentioned above. Analysis shows that it always refers to *intensity* of *belief* of an individual or to conventional beliefs of groups. Attempts are often made to assign a numerical estimate to such probabilities. In doing so, one of two courses is usually employed. Thinking again of the statement: it will probably rain tomorrow, a measure can be obtained by studying the weather today and then referring to a record of meteorological data for this locality[7] and finding the relative number of cases in which rain has ensued on a day prior to which the weather agreed with today's. But in this procedure we are

[7] For an analytic discussion of the meaning of measurement cf. the next section.

clearly giving up the subjective idea of probability as well as the individuality of the day "tomorrow," which now merges in the statistical aggregate of days characterized by the property that the previous day had weather like the present. Proponents of the subjective view are not always aware of the necessity of a reversion to the frequency concept, typified by the preceding example, which necessity invariably arises when any scientific application is to be made. The other course sometimes adopted for making *a priori* probabilities statable in numerical terms is to count causes or reasons for or against the occurrence of the event in question. Thus it might be said: 7 causes indicate rain tomorrow, only 2 indicate clear weather, therefore the probability that it will rain is 7/9. But who can determine the relative weights of these causes, or who can say that I have counted the correct number of them (for any given cause can be split up into a number of causes)? In some instances one can say: 7 available theories favor the event, 2 theories oppose it, hence the probability is 7/9. But then, what are we to do with that infinite number of theories, also favoring or opposing the event, which have not yet been invented, or what are the antecedent probabilities of being true for those that have been taken into account? We return therefore to the initial statement regarding subjective probabilities and phrase it thus: *Probabilities referring to intensity of belief, or degree of completeness of knowledge regarding a single event, are intrinsically unmeasurable.*

Now it is a fact that the student who holds the objective view concerning states is practically forced to couple it with the empirical frequency concept of probabilities, whereas those who incline toward the subjective interpretation of states almost invariably adopt the subjective idea of probability also. The internal compulsion for this alignment is unmistakable. A person willing to assign to a physical system a state independent of his cognition would find it natural also to allow this state numerous modes of empirical manifestation, whereas the knower of a subjective state has room only for greater or lesser certainty of his knowledge. He would say: Now I have made a calculation which makes my knowledge of the electron's momentum certain

to the degree 1/2, my knowledge of its position certain to a similar degree; now I have measured the electron's momentum and I know it with complete certainty, while my knowledge of its position is now extremely uncertain, etc. In his opinion this is described in the changes which the state function undergoes.

It must already be apparent that the choice of interpretation of physical axioms is not always a matter of logic, but often of personal preference. So here the correlation of views is not entirely rigid. A mixture is possible in the sense that the last named individual might, while maintaining his fundamental stand with regard to the subjective character of states, nevertheless submit his knowledge to experimental tests and thus affix to it an indirect objective measure of certainty. But the view which then results is needlessly complicated and approaches the former in all essential respects. For this reason it does not seem worth recommending. The possible number of combinations of aspects so far presented in this report is already so large that some curtailment becomes imperative.

III. THE STATISTICAL CHARACTER OF PREDICTIONS

The considerations in this section are irrelevant to the subjectivist who has adopted the *a priori* probability idea. We wish here to deal with the question: granted that the probability predictions which arise in quantum mechanics refer to a statistical aggregate in the spirit of the frequency theory, what type of aggregate is meant? It is clear that it must be an aggregate of physical observations, indeed in the ideal limit an infinite number of observations, or measurements.[7] Surveying the field, we find after little search that three types of collections of observations present themselves:

1. Numerous simultaneous observations of a given quantity when the system is in a given state;
2. Numerous observations, or a single collective observation, on a physical assemblage of many similar systems in the same state;
3. Numerous repeated observations on the same system, the state in question being reprepared before each observation.

The first of these is only a speculative possibility, for there are many types of observation which can be carried out but once and permit no simultaneity with any other measurements even of the same type. Hence our choice is reduced to possibilities (2) and (3).

The contrast between these is best illustrated by an example. Let the state function for the system "electron" be such as to imply: the probability of observing its position in a certain specified volume is 1/10. According to (2) this means that if I take 1000 electrons all in the state under consideration (we suppose at present that I know how to prepare this state) 100 of these electrons will lie in the specified volume. According to (3) it means that if an electron is in the state in question, I may find it in the specified volume or not, but if I prepare the state 1000 times, I shall find it there approximately 100 times. In (2) the probability aggregate is a collection of observations on a physical assemblage, in (3) it is a collection of observations on an ideal ensemble consisting of an infinite repetition of a set of physical operations. The same fundamental distinction occurs at one other place in theoretical physics, namely in the approaches to thermodynamics by Darwin and Fowler on one hand, and by Gibbs on the other.

The decision here to be made clearly involves the question whether or not quantum mechanical reasoning is at all applicable so single systems. If it is not, (3) must be abandoned. Otherwise both might be acceptable, but (2) might be a consequence of (3). Now it is certainly true that in most physical observations dealing with atomic phenomena indirect means are employed, and the number of systems affected is very great indeed. The energy of an atom can be measured for instance by allowing it to radiate and then analyzing the radiation. But when the spectrum is to be photographed the radiation from a single atom is far too weak to register itself. Hence a great number of atoms must be admitted. It is found that the spectrum always contains many different frequencies. Does each atom emit all these frequencies, or does each emit a single frequency, the different ones coming from different atoms? Experiment does not provide the

answer directly, but we shall find it by considering another situation.

Quantum mechanics predict for the coordinate (probability) distribution of an electron which has passed through a narrow slit, a diffraction pattern, i.e. well defined maxima and minima in a plane behind the slit. If, in order to test this result, a beam containing many electrons is used, the pattern can be observed by means of appropriate devices, like ionization chambers. By this experiment the question whether a large number of electrons is necessary to produce the probability pattern can not be decided. It is feasible, however, to make the electron beam very weak, so weak in fact that the electrons constituting it are widely separated in space and cannot be supposed to interact with one another. Under that condition the predicted distribution is also observed, but now as a frequency distribution among the positions of capture of the individual electrons, the captures being altogether independent events. The conclusion is therefore inescapable that the statistical feature of this process is attached to the individual electron, and that, even if the same state were prepared again for the same electron, it, too, would show a dispersion of positions. And this means that quantum theory deals with an aggregate characterized by (3), not necessarily by (2).

Several other experiments could be adduced to confirm the proposition that the theory is applicable to single systems. We shall content ourselves with one further point. It is well known, and demonstrable in many ways, that a light signal which is emitted in a very short time cannot be monochromatic but must have a wide spread in frequencies. Modern theory bids us consider the light flash as a photon, a *single* photon if it is sufficiently weak, and its frequencies as being proportional to energies. If now that well-known fact is to be preserved, we must, in quantum mechanics, associate many different energies with a single photon; we must assume that a photon in a state defined by its sudden generation must exhibit sometimes one, sometimes another energy to the observer.

We can now say what happens, presumably, when many atoms emit a spectrum. *One* atom would generate the entire spectrum

if it were given sufficient time to register its radiation on a photographic plate. Of course a single photon would fall on only one spot of the plate, but another photon would hit another spot, and so on, until finally the whole plate is blackened in accordance with predicted probabilities. The supposition is that the atom is returned to the same state before each act of emission, and this may be done, for example, by exposing it constantly to another suitable source of radiation.

It is clear, then, that the statistical aggregate in question can not be defined by (2) at the exclusion of (3). The latter is certainly a correct interpretation. On the other hand, no one will deny that possibility (2) is somehow more graphic and more plausible. The reason for this is not far to seek. It is simply because the older classical statistical theories have familiarized physicists as well as philosophers with the notion of assemblages (e.g. gases) which exhibit probability features only by virtue of possessing hosts of constituents. There every constituent obeys strict dynamical laws, statistics arise from our ignorance of certain parameters, such as the initial positions and momenta of all the molecules, and the argument goes that if these parameters were known the use of probabilities would be unnecessary. In quantum mechanics, as we have shown, the situation is altogether different; no search for hidden parameters in the ordinary dynamical sense can ever undo the probability thesis—unless the theory itself is abandoned or essentially modified. This follows simply from the fact that in quantum theory the single system, which is the carrier of the statistical aggregate, can by definition not be further decomposed into entities with unknown properties.

If (3) defines the aggregate to which the theoretical probabilities refer, what about the aggregate (2)? A moment's reflection will show, we believe, that it is a special case of the former, provided we are willing to make a slight but very significant change. In (3) we spoke of repeated observations on the *same* system. There are many reasons for believing that one ultimate system, like an electron or a proton or a neutron, is physically indistinguishable from another system of the same kind. Therefore it seems permissible to loosen the strictness of the word *same* into a sort of

of promiscuous identity, designating by the same system merely a system of the same kind. The collective measurement on a physical assemblage of similar systems, contemplated in (2), may then be regarded as a sequence of almost simultaneous measurements on the "same" system in the new sense of (3). Item (2) thus becomes a special case of item (3). But item (3) expresses the basic issue.

IV. THE THEORY OF MEASUREMENTS

A. *Meaning of measurement.* The usual way to theorize about measurements is to select, for no obvious reason at all, a specific type of experiment, analyze it very fully, and then generalize the results without inquiring very much whether these results fit the multitude of other measurements that might be considered. The experiments chosen are mostly imaginary ones, a feature which of course detracts in no way from the value of the example as long as the imaginary procedures are permitted by known physical laws. Heisenberg's experiment for determining the position or the momentum of a particle by means of a γ-ray microscope, and Bohr's slit experiment have perhaps borne the brunt of this procedure. Not as common but yet quite popular, particularly among mathematicians, is a mode of inquiry which starts with quantum mechanical theory, considers what *ideally* a measurement ought to be, and then manages to find one which fits the prescription. In studying J. von Neumann's excellent book[8] on the foundations of quantum mechanics we have had the impression that this method was being employed. What makes the method seemingly successful is the *wealth* of existing *types* of measurement; it insures that any prescription can be filled.

Let us here try to avoid these procedures, let us begin by analyzing empirically, and on a sufficiently broad basis, what the physicist means when he says he makes a measurement. The breadth of the issue is at first bewildering, but it seems that the following definition covers it completely: *a measurement is any physical operation by means of which the numerical value of a*

[8] von Neumann, Mathematische Grundlagen der Quantum mechanik.

physical quantity can be determined. This wording is not meant to suggest that physical quantities are always or sometimes "*real*," or that they possess at any instant unique numerical values. We know that they do not, and we refuse as physicists to join the irrelevant altercations about reality. What matters is that there are generally accepted physical operations whose outcome is associated with a certain number, and this number is commonly referred to as the value of some quantity. All dangers are avoided if the reader thinks of "value of a quantity" as a figure of speech. The definition shows that a measurement is different from a physical operation and an observation, measurement being more specific than either of the last terms, while an observation differs from an operation in being a purely passive act of perception. A measurement usually consists of an operation plus an observation, but this need not be the case.

The present purpose is best served by leaving aside altogether measurements on systems of the immediately perceptible domain, i.e., on large scale bodies, and to concentrate our attention at once upon the more problematical part of our question, the measurement on *atomic* systems, atomic in the original sense of the word. Our main endeavor is to show how rich is the field of possible procedures.

The simplest quantity to be measured on any system is perhaps its position. Among the possible ways of effecting a position measurement one seems to be most commonly employed: the method of absorption. If the system is an electron, Faraday cages or similar devices might be stationed at various places, and the place of the one which registers is the place where the electron has impinged. If the system is a photon, a photographic plate is used, and the place of the photon's impact is indicated by certain chemical changes. (We are not concerned at present with limits of accuracy of measurements.) Next let us turn to the momentum of a single system, say an electron or a photon. Perhaps the simplest way of measuring it is to allow the particle to be diffracted by a suitable grating and then to determine its impact on a plate, i.e. its position in the manner just discussed. The energy can of course be found in a similar fashion. These

experiments are more complicated than the foregoing ones inasmuch as they require the addition of another stage or operation to the position measurement, and this seems to be a typical feature of most measurements. It is to be observed that the phenomenon of diffraction alone is *not a momentum measurement* in accordance with our definition, for unless it is coupled with the absorption of the photon on a plate no number can be ascertained. We deem this fact of some importance despite the carelessness with which the matter is often treated, and we shall have occasion to return to it in another connection.

It would be folly to suppose that all physicists will consider the simple measurements here chosen for discussion as more basic or more representative than others. The point to be made is that they are *bona fide* measurements of very common occurrence. They present an interesting feature: after the measurement the system can no longer be said to be present as an identity. This is particularly true in the case of the photon which after absorption ceases to function as a physical system altogether, while the electron may in a sense be said to be "still there," but has become one of a large assemblage of indistinguishable electrons inside a metal. We note, then, that *some measurements annul a system*, others render them of no physical interest with respect to the measurement in question. Further examples of measurements which annul the system would be the determination of the place of a neutron (by nuclear capture) and certainly that of a neutrino, if this should prove to be a useful construct. Cases of this type arise of necessity when systems are introduced which can be created or destroyed.—Throughout this discussion the language of classical physics has been used. In terms of it we might state the matter thus: the classical fate of systems during measurement is not describable in a uniform way; an attempt to describe it completely would introduce irrelevancies. We shall see that quantum mechanics, with its altered definition of states, relieves us of all embarrassment in this connection.

The number of legitimate measurements in physics is theoretically unlimited, as is easily seen from the fact that every new discovery, every new verified theory provides new opportunities for

measuring old quantities. For instance, a Wilson cloud track would not have been a means for measuring the energy of an α-particle before Wilson's discovery of the condensation of water vapor on gaseous ions. Nor would Heisenberg's γ-ray microscope have been a suitable device for measuring an electron's position before the Compton effect was known. What is now termed a measurement would then have been merely an experiment. In view of the derivability of new kinds of measurement from special discoveries, the temptation is strong to standardize the theory of measurement by picking out a certain basic group, perhaps to be called the primary group, on which may then be built an unlimited system of secondary measurements. Sucn an analysis would be extremely desirable for logical as well as linguistic reasons, but will not be attempted here because the selection of primary measurements would be an arbitrary undertaking.

Besides measurements, there is another very important class of physical operations: *the preparation of states*. We are not at this moment interested in such operations, but we do not wish measurements to be confused with them. For example, when electrons are made to pass into a magnetic field, a new state with respect to electron spin has been produced, *but the spin has not been measured.*

A word might be said about the compatibility of measurements. The literature abounds with statements to the effect that simultaneous measurements on complementary quantities (in Bohr's sense) are incompatible. If this is interpreted as meaning that they cannot be made the statement is either without point or it is wrong. For in that class of measurements which annul a system no simultaneous ones are possible at all and the proposition seems void. On the other hand there is nothing to prevent an inquisitive observer from illuminating an electron simultaneously with hard and soft quanta, and if, according to the orthodox view, the hard quantum on being scattered tells the electron's position and the soft one its momentum (Doppler effect), two "incompatible" measurements have been performed. We know that the results are not to be trusted and that, on repetition, the measured values would scatter greatly, but the example does

constitute an exception to the *general* claim of incompatibility for (simultaneous) complementary measurements. What the phrase should mean is that such measurements have a predictable spread.[9]

There appears to be a tendency to regard quantities of a physical system which can be measured collectively *without spread* in the previously defined statistical sense, as *real* quantities, others being unreal. This is the most peculiar use of a metaphysical term, a term which would be objectionable in physics even if it were used more correctly. It springs very clearly from a failure to abandon classical notions where they should be dismissed. In classical mechanics, when a definite ultimate state is given, an elementary quantity which refuses to yield the same value every time it is measured is a self-contradiction, is unimaginable. In quantum mechanics such things do occur, and the success of the formalism has forced many classically-minded physicists to accept it. But their displeasure still prevents them from acquiescing completely, and they feel better when they are able to give things a private little knock by calling them unreal.

B. *Measurement and Uncertainty.* The mathematical meaning of the "uncertainty principle" is perfectly clear and requires no comment. To wit: when any state function is given, the uncertainty—a term which has no precise accepted significance but can be defined suitably—may be computed numerically. Ambiguity prevails only as to the interpretation of the numbers which so result.

Here again account has to be taken of the two possible meanings of probability. On the subjective view, the uncertainty affects a single measurement, but it can in that case be *nothing more than a qualitative subjective estimate, clad in numerical form but incapable of verification, of the assurance with which certain results may be expected.* Every other interpretation would at once revert to the position which envisages the single measurement as the constituent of a statistical aggregate, and this would

[9] In making such predictions the exact meaning of the word "simultaneous" must be known. The results of the measurements depend on the order in which they are performed because the operators in this case are not permutable.

necessarily be the one discussed in section III, (3). The usual statement according to which an uncertainty represents an error is meaningless on this view, for a single measurement contains no error, or, more explicitly, no verifiable error. The present position, if maintained consistently, is by no means illogical; but it fails to utilize a wealth of formal structure potentially present in quantum theory.

If, on the other hand, the probability concept is accepted in its empirical sense, uncertainty can truly be interpreted as an error. The uncertainty principle (for the sake of definiteness let us think of it in the form $\Delta p \cdot \Delta q \geqq \frac{h}{4\pi}$) then relates the spread, Δp, occurring in numerous momentum measurements on similarly prepared systems, to the spread Δq in corresponding position measurements. On this view the principle becomes far more explicit, it exposes itself in a simple way to empirical verification, which, by the way, lends it the strongest support. The inequality sign appearing in the usual form of the relation, or the sign $\sim$ which is also frequently used, does not imply that Δp or Δq are not known with precision; indeed in any given case both Δp and Δq can be computed exactly from a knowledge of the state function; and the computed values are independent of the kind of measurements to be used for their determination.

Most physicists consider it very important to secure a plausible classical explanation of the uncertainty relations, which fundamentally are nothing more than a by-product of the quantum mechanical description of nature. There is indeed no harm in seeking such an explanation, but in doing so one should be aware that the procedure involves basically a fusion (and often a confusion) of incongruous physical concepts: the classical and the quantum mechanical ideas of a state. In this connection it is well to remember that the uncertainty relations were implicitly used before they were discovered. The common claim is that they express the uncontrollable error introduced into physical description by the interaction between system and measuring devices in the act of measurement. The physical facts underlying this assertion are undoubtedly correct, but the statement is

unfortunate in the light of a more meticulous analysis of the meaning of measurement. We have seen that the theoretical uncertainties are completely independent of the choice of measuring devices, whereas measurements affect a system in a perplexing variety of ways. Uncertainties are in a very definite sense inherent in a state even before measurements have been made. The correct statement therefore is that *the uncertainty relations reflect the interaction between system and apparatus when the state is being prepared,* not when it is measured. A closer inspection of the arguments presented in the literature for the former loose assertion reveals that its defenders perhaps unwittingly mean the latter. Logically, however, there is a difference, for the former position inevitably leads to a theory which counts the disturbing interactions twice, once in the preparation of the state, once in its measurement. What quantum mechanics actually does is to stop all inquiry at the act of measurement.

We feel that the matter is really quite simple and that most argumentation on this issue has resulted from a failure to define concepts clearly. The most common artificial complication arises in this way. Numerous measurements (not all) have of course the property of preparing a new state, a fact which is of no interest if I agree to regard the act in question as a measurement. If, however, by a mental lapse I forget to efface all interest after this event and continue to question what happens, then I have contradicted the original premise according to which I was making a measurement and am regarding the act as an operation preparing a state. The practically-minded physicist will probably regard these distinctions with mild amusement—but we are, after all, dealing with the finer points of modern theory.

The problem of causality, which naturally raises its head at this place, has been dealt with previously[10] from this point of view. We have no cause to modify the conclusions then presented.

C. *Does a measurement produce an eigenstate?* The reader will recall that an eigenstate of an observable quantity is a state for

[10] H. Margenau, Journal of Phil. of Science, 1, 133 (1934).

which the measurement of a single value of that observable quantity can be predicted with certainty. In other words, the probability distribution with respect to that observable, which can be computed from the state function, degenerates into zeros for all values but one. Now orthodox theory claims[11] that a measurement, performed upon a system in an arbitrary state, immediately and suddenly converts this state into an eigenstate with respect to the quantity measured. This change is held to be indescribable by any theory and essentially unpredictable. Troublesome questions as to the length of time which must be allowed for this sudden change, whether it takes place discontinuously in zero time or whether it is somehow related to the kind of measurement enacted, have already arisen but are left unanswered because quantum theory is felt to be not sufficiently complete to deal with them. This doctrine recognizes two possible ways in which a state function may be altered; first, continuously in accordance with the Schrödinger equation, the change being uniquely predictable; second, in an unpredictable way when a measurement is performed. The eigenstate thus generated is of course the one corresponding to that particular value of the observable which the measurement has yielded. On this theory of unpredictable transitions is often based the assertion that quantum mechanical description is an essentially acausal one.

Let us view this matter in the light of the analysis conducted in this paper. It is then perfectly clear that our attitude here is decided by the fundamental choice made with respect to the interpretation of states. *If we adopt the subjective view*, according to which a state function is a symbol merely for knowledge, and couple it with the subjective view of probabilities—and *only* if this choice is made—*the theory of acausal jumps is indeed correct*, for a measurement converts uncertain knowledge into certainty. It is interesting to observe that this change occurs as the result of a *single* measurement. Therefore the probability interpretation attending the whole process must be the one which endows the probability of single events with meaning, and it entrains

[11] Cf. for instance von Neumann, Mathematische Grundlagen der Quantenmechanik, Schrödinger, Naturwissenschaften 23, 823 (1935).

those consequences, such as lack of measurability, which we have discussed in section II. We have already indicated that in our opinion such consequences are unfortunate, but if they are accepted, the orthodox view is entirely logical.

On the other hand, let us see what happens if we regard states in the objective sense, and measurements as revealing states in a statistical manner, the statistical aggregate being the one exposed in section III. *The theory of acausal jumps is then decidedly to be rejected.* For in the first place a single measurement allows no inference as to the state at all. In the second place, we have seen that measurements, if described in classical language, affect systems in such a large variety of ways, sometimes even annulling them completely, that they cannot be said to produce an eigenstate of an observable in a uniform manner. It is the great merit of quantum mechanics that it permits a description of experience without forcing us to take account of the sometimes disastrous results of a measurement. Classical physics required the investigator to be ever on the guard for disturbances occurring when he made his measurements. He maintained his vigilance in the mistaken belief that such disturbances could be controlled, but had to capitulate when this belief was shaken. The new theory permits us to forget these difficulties completely by incorporating a suitable compensation mechanism in its fundamental structure: the method of describing measurements statistically. On the objective view, the assertion that a measurement produces an eigenstate is precisely as meaningful as the contention that, after cracking and eating a nut, I still have a nut, but in its cracked and eaten state. In an earlier paper[12] the present attitude was briefly formulated, but sufficient reasons for its acceptance were not given.

Perhaps it is well to remark in some detail on a few arguments which often serve as a basis for maintaining the objective view of states, together with the theory of acausal transitions. One, illustrated with the use of an example, runs as follows: I wish to measure the state of polarization of a photon, about which I know

[12] H. Margenau, Phys. Rev., 49, 240 (1936).

nothing initially. For this purpose I send it through a Nicoll prism, an analyzer. If it passes through I have made the measurement, for I then know its plane of polarization to be the plane in which the Nicoll transmits. But if I station another analyzer immediately behind the first, with its axis parallel to that of the first, I am absolutely certain that it will go through the second Nicoll also. Hence the first measurement has produced an eigenstate with respect to the photon's state of polarization. The error lies in forgetting that I have not made a measurement until I have ascertained, by catching the photon on a plate, that it has gone through the Nicoll. The error is simple but essential, it springs from a hazy conception of what is meant by a measurement, and this, incidentally, is the cause of many similar difficulties.

Occasionally one finds the position, already shown to be untenable in section III, that quantum mechanics deals with actual assemblages of similar systems, like a swarm of electrons, in some state. If a measurement is then performed, a sub-assemblage is selected from the former group of systems, this sub-assemblage comprising all those individuals, which have the measured property. It is this new, smaller assemblage to which the state function after the measurement refers, and this is clearly in an eigenstate. So goes the contention. Three comments should be made with regard to it: (1) quantum mechanics is capable of dealing with single systems and is not limited to assemblages; (2) very few measurements have the property of selecting sub-assemblages, on which the above argument rests; (3) a state function in quantum mechanics refers always to the same number of individual systems, i.e., to the same assemblage. This argument is even weaker than the foregoing one and is indeed also less common.

The real backbone of the theory of acausal jumps is probably to be found, however, in a peculiar mathematical attitude which gives way to a tendency of securing physical counterparts to mathematical notions whether these counterparts fit into the physical scheme or not. To wit: the mathematical theory of quantum mechanics is full of operators, that is, to every physical observable there corresponds an operator. But very few of the

available mathematical operators are accommodated in this way, are ennobled by being physically established. There is in particular a very beautiful and general class of operator, the projection operator (terminology comes from the theory of Hilbert space) which converts a function into what amounts to an eigenstate-function. This operator fairly cries for physical accommodation. What is more suggestive than to associate it with measurement in general? This correlation, once tentatively made, finds support in the fact that there are, as we have seen, *some* measurements which, if they are wrongly interpreted as operations producing states, do produce known eigenstates. Hence the theory which we are critizing. We have here encountered another example where a theoretical suggestion, supported by a few empirical instances, is blindly generalized into an all-embracing law.

The view that a state function jumps unpredictably during measurement may lead to monstrous consequences of which only one will be considered. Unless a measurement is cleanly separated from other physical operations—and this separation, as we have seen, involves a rejection of the jump theory—one is induced to reason as follows: a measurement is, after all, the result of an interaction between the system and some physical apparatus. If, then, this same physical process is allowed to take place but without the understanding that it shall constitute a measurement, the state function describing it will develop continuously in accordance with the Schrödinger equation; if the process is regarded as a measurement it will change abruptly and indeterminately. Where is the line to be drawn? One might for instance be looking at a recording device and, while day-dreaming, fail to take conscious cognizance of the registration. All *physical* processes, as the term is ordinarily interpreted, are the same as if he were taking conscious notice of the result, but in one case the state function develops continuously, in the other it changes abruptly. This conclusion is clearly not interpretable unless we are willing to say that the state function means knowledge. If the absurdity is justified, as is sometimes done, by the observation that processes in the human brain do matter and that

the ego is to be introduced into the scheme somewhere, the only significant reply is, we believe, that quantum mechanics does not as yet pretend to be a psychological theory. As such it would have to show a little more competence in the purely psychological realm. Again the solution of the apparent dilemma is perfectly simple: There are no acausal transitions *of states*; if I consider the experimental procedure as a measurement I should refrain from talking about it as though it were not. I must then stop my description of the state after taking cognizance of the measured value. On the other hand, if the procedure is not a measurement I can certainly (at least in principle, actually the description it very difficult and usually impossible) continue to trace the state function as long as I desire in definite fashion; the registration which has occurred incidentally in this procedure does not, as we now know, determine the state function completely, nor could it in general have been predicted from a complete knowledge of the state function.

Of some interest in the present context is a mathematical process known as the "method of reduction of the wave packet." Its exact nature will not be discussed in great detail.[13] Its usual application to systems containing two elementary particles makes use of the "jump theory" in this way. The state function of the pair is, by reason of a past interaction, such that it can not be written as a product of two functions each containing the coordinates of but one particle, but appears as a sum of such products. When a measurement on one particle is made, the state function corresponding to that particle is known: it represents the eigenstate belonging to the measured value. But this function appears in the original sum as multiplied by a coefficient which contains only the coordinates of the other particle, and which can therefore be regarded as its state function after the measurement. Thus, it is contended, a measurement causes the originally mixed function to reduce itself suddenly to a simple form, thus "resolving the wave packet."

On the present view, no such event happens; it can only be

[13] See the appendix, II, for those parts which are here of interest.

shown—and this is done in the appendix[13]—that there exists a peculiar correlation between the probabilities for the various results of measurements. It is this: When suitable measurements on the two particles are made, *the probability for the occurrence of a pair of values is zero unless the pair is the one which corresponds to the two eigenfunctions of the reduced package.* The physical consequences are the same as on the customary view, but the result appears now in less metaphysical language. The method of the reduction of the wave package remains in good practical standing, only its interpretation requires greater care.

It is hoped that the analysis, which is herewith concluded, may contribute toward a more precise understanding, a deeper appreciation of the internal logical consistency, and a clearer linguistic formulation, of recent physical theories.

APPENDIX

I. The state function ϕ of the electron, if it were known, could be expanded in terms of the eigenfunctions ψ_λ, of any other operator A, as follows:

$$\phi = \sum_\lambda a_\lambda \psi_\lambda \tag{1}$$

The a_λ are complex numbers, to be interpreted as follows: the square of the absolute value of any particular one, say a_k, is equal to the probability that a measurement of A yields the k^{th} eigenvalue A_k.

Suppose now that ϕ is not known, but that all $|a_\lambda|^2$ are known from experiments on A. The value of every a_λ is then uncertain with respect to a factor of absolute value 1. If we attempt to construct eq. (1) we can at best only get

$$\phi' = \sum_\lambda a_\lambda e^{i\delta_\lambda} \psi_\lambda \tag{2}$$

with all the δ_λ's unknown, except that they must be real.

The important point is that (1) predicts a different probability distribution for another observable with operator B, than does (2). For if χ_μ is the sequence of eigenfunctions belonging to B, then the probability of measuring the eigenvalue B_k when the electron is in the state ϕ will be

$$\left| \int \phi \chi_k^* d\tau \right|^2 = \left| \sum_\lambda a_\lambda \int \psi_\lambda \chi_k^* d\tau \right|^2 \tag{3}$$

But when the system is in the state ϕ' which is experimentally indistinguishable from ϕ, this probability is

$$\left| \int \phi' \chi_k^* d\tau \right|^2 = \left| \sum_\lambda a_\lambda e^{i\delta_\lambda} \int \psi_\lambda \chi_k^* d\tau \right|^2 \qquad (4)$$

(3) and (4) are equal only under two conditions:

(a) when all δ_λ are equal

(b) when the sum in (4) reduces to a single term.

Case (a) is an accident which is not under our control. Case (b) can arise in one of two ways: *Either* the integral $\int \psi_\lambda \chi_k^* d\tau$ is zero for all but one value of λ. Then χ_k is essentially the same as one of the ψ's, and we are measuring the observable A over again. It is small wonder that we can predict what we already know! *Or* every a_λ is zero except one. This is the situation discussed in the text. It means that the first operator A was one for which the measurements did not spread.

II. Let the state function after the interaction be

$$\psi(x_1 x_2) = \sum_\lambda v_\lambda(x_2)\, u_\lambda(x_1) \qquad (5)$$

Here x_1 and x_2 denote the coordinates of the two particles, respectively; u_λ is an eigenfunction belonging to the eigenvalue A_λ of the operator A; v_λ is (a constant times) an eigenfunction belonging to the eigenvalue B_λ of the operator B. A "biorthogonal" development like (5) is sometimes possible,[14] it has been made the basis of certain metaphysical conclusions by Einstein, Rosen and Podolski[15] which we shall not discuss further.[12] We are here not interested in other cases. The probability that a simultaneous measurement of A on particle (1) and of B on particle (2) shall yield the pair of values A_i and B_j is, by the principles of quantum mechanics,

$$\left| \iint \psi(x_1 x_2)\, u_i^*(x_1)\, v_j^*(x_2)\, dx_1 dx_2 \right|^2$$

$$= \left| \sum_\lambda \int v_\lambda(x_2)\, v_j^*(x_2)\, dx_2 \int u_\lambda(x_1)\, u_i^*(x_1)\, dx_1 \right|^2$$

$$= \left| \int v_i(x_2)\, v_j^*(x_2)\, dx_2 \right|^2 = c\delta_{ij},$$

[14] Cf. Schrödinger, Proc. Camb. Phil. Soc., 31, 555, (1935).

[15] Einstein, Rosen, and Podolski, Phys. Rev., 47, 777 (1935).

where c is a constant and δ_{ij} the Kronecker δ-function. We are supposing for simplicity that all states occurring here are non-degenerate. It is thus seen that the probability of measuring A_i and B_j is zero unless i and j characterize two functions paired in (5). This is all that matters in any application of the method of resolving the wave packet. The conclusion that the state function makes an abrupt transition is quite irrelevant and avoidable.

REFERENCES

A. Einstein, B. Podolski, and N. Rosen, Phys. Rev., **47**, 777 (1935).
E. C. Kemble, Phys. Rev., **47**, 973 (1935).
A. E. Ruark, Phys. Rev., **48**, 466 (1935).
N. Bohr, Phys. Rev., **48**, 696 (1935).
E. Schrödinger, Naturwissenschaften, **23**, 807 (1935).
E. Schrödinger, Naturwissenschaften, **23**, 823 (1935).
E. Schrödinger, Naturwissenschaften, **23**, 844 (1935).
E. Schrödinger, Proc. Camb. Phil. Soc., **31**, 555 (1935).
H. Margenau, Phys. Rev., **49**, 240 (1936).
W. H. Furry, Phys. Rev., **49**, 393 (1936).

Yale University,
New Haven, Conn.

Objectivity in the Social Sciences

BY

ELEANOR BISBEE

TO BE scientific is to be objective. This is a sine qua non in the social scientists' efforts to keep their researches on a par with those of the natural scientists. The extreme differences of subject matter in the two fields, however, make it difficult to maintain an even standard of objectivity. Whether objectivity is defined in the strict Cartesian sense or in a broader sense also makes a difference. The Cartesian meaning threatens to enslave the social sciences to Physics as the Lord of All Sciences, and this possibility is so familiar that it needs but brief comment. A broader interpretation of objectivity meets with complications which are only inadequately recognized as yet, and which may call for reinterpretation of the scope of science or else may open up new and independent avenues of opportunity beyond the limitations of science.

Modern science originally was an objective study of the primary qualities, extension and motion, which belonged to the object as such. Man's knowledge of those qualities, it was believed, in no way alters the objects, and so the facts thus known must be the same for all knowers. This clearly excluded from the field of science all Cartesian secondary qualities, namely sensations, and likewise all emotions and desires and also all the more recently termed "tertiary" qualities or human values. Today, sensations and emotions and values are the phenomena about which the social scientists would like to be scientific, which, for them, means to be objective. The social scientists have thereby set for

themselves the paradoxical ambition to be objective about the subjective aspects of life.

One way to resolve this paradox is to eliminate the subjective distinction by reducing all secondary and tertiary qualities to the objective, primary ones of extension and motion or to functions of these, and then to give quantitative explanations in the terms of matter in motion. A. B. Wolfe, an economist, who is a typical exponent of this form of objectivity in the social sciences, makes the characteristic statements that, "... a scientific psychology is possible because man is a part of nature and in no way an exception to its laws.... His behavior is as mechanical in the last analysis as that of any other part of nature."[1] Undeniably, this falls in line with the most orthodox tradition in physics insofar as it concedes that strictly mechanistic phenomena are the proper subject matter of science. The reduction of human behavior to such purely mechanistic relations has, as a rule, come to mean acceptance of Behaviorist psychology. Surprising dogmatism is revealed, however, in the unquestioning acceptance which many social scientists give to this mechanistic psychology as an all-sufficient explanation of human life. It may be sufficient to meet the standards of physics. Is it sufficient to account for all that makes man so unique and so interesting to himself? That is a moot point and psychologists must settle it. If they settle it finally in favor of Behaviorism as the sole and total explanation of human phenomena, good Cartesian objectivity will have been preserved, subjectivity will have been annihilated, and the way of Physics will be the way of all sciences worthy of the name at all.

The other way to resolve the paradox is to have a broader interpretation of objectivity, and this is the way of a growing majority of social scientists. The broader meaning simply puts the emphasis on what was a consequence of the Cartesian definition, namely that objective facts are alike for all knowers and so objectivity is verifiability by all equally competent persons. Objectivity, in this sense, encompasses observations of mechanistic phenomena, and it also includes formal logic, mathematics

[1] Tugwell, R. G., Editor: "The Trend of Economics," p. 479.

and any sound deductive reasoning, every step of which can be verified by any or all people who are capable of duplicating the reasoning process. The prolonged success of modern science rests upon confidence won by this kind of objectivity. The legitimacy of the universal proposition that all scientific results are verifiable by equally competent persons is obvious. Pasteur put no limits to the privileges of any and all impartial observers to test his empirical findings by this standard of objectivity. Einstein is equally willing to submit his rationalistic speculations to the same test of verification by impartial thinkers, and to have impartial observers check any predictions which he deduces for nature. Any scientific thinkers invite this test.

Granted then: All scientific knowledge is verifiable by equally competent persons. Among the hordes lured to study and practise in the social sciences, there is an unfortunate tendency to illicit conversion of this proposition. Too many social scientists believe that it logically follows that everything verifiable by equally competent persons is scientific knowledge. This conversion would, of course, be valid, if the original proposition were properly a definition of science, as many people naïvely suppose, but it is not that. It merely states an important characteristic of science. That characteristic may not be confined to science alone. There may be some objectivity which is not scientific. One illustration should suffice to make this point clear.

In the naïve faith that objectivity is invariably scientific, schools often boast of their "scientific methods" because they give "objective" tests, which can be scored uniformly by all competent persons. Occasionally, the suggestion is made that advancement of teachers should be as "scientific" as promotion of pupils, and, therefore, that teachers should be given objective tests to determine their ranking in order of competence. To cite one specific instance of this not uncommon attempt, such tests for teachers were proposed in Cincinnati, Ohio,[2] but they were rejected on the grounds that too many heterogeneous factors enter into teacher-competence for the mere objectivity possible

[2] Reports of the Board of Education and the commission of investigation, Cincinnati, Ohio, spring of 1936.

in tests to constitute a scientific basis for prediction of merits. There was no doubt at all that certain teaching qualities could be tested objectively, but, when tested, the results still would not insure scientific finality in the matter of promotions. Those same teachers, however, were expected to believe that pupil-competence, i.e. ability to pursue studies farther and to merit promotion, can be objectively tested, and that, therefore, the promotion of pupils is on a "scientific" basis. Apparently what makes a difference is the type of thing to be tested, and objectivity in some cases falls short of testing all that needs to be known or it brings out the wrong information, and in either case, the mere fact that the results are objective is not alone sufficient to make them scientific.

If there may be some objectivity which as such is not scientific, this leads to the question, what does identify the objectivity which is scientific? For Galileo and his successors, the homogeneity of matter in the form in which they studied it, was sufficient guarantee that duplicable conditions would produce duplicable results, and thus would be objectively verifiable in the future as well as in the present. With heterogeneous material, there is no such guarantee that objectivity will have scientific value. With secondary qualities for example, all investigators to date may have duplicated their tests, but any generalization of these findings is a complete induction merely stating what has been observed, and it is not the sort of predictive induction which science demands. The very next experience of secondary qualities may be different, for the nature of the material is such that each additional duplication of results brings closer the inevitable exception. Were this not so, the material would be dependably homogeneous.

Social scientists know that their material is far from homogeneous, although they insist upon trying to treat it by methods uniquely successful in dealing with strictly homogeneous material. When they cannot depend on the homogeneity of their material to validate their objective findings, so that they can properly say, "This result is dependable," how do they validate their objective tests? The answer to this question brings out the highly signifi-

cant but seldom vaunted fact that some of the most successful validation of objective tests depends upon purely subjective judgment.

Personality tests are an extreme illustration. In most of these an examinee is placed on a scale between opposite traits such as "very steady and dependable" and "very erratic and undependable." This, it is said, should be done by expert raters. Raters are people in a position to know the examinee's character, maybe a teacher, a foreman, a personnel secretary or anyone who sees the examinee constantly in the course of his usual activities. The expert rater is trained to guard against the so-called "generosity error" which gives each examinee too much benefit of the inevitable doubt as to how far to the left or right to place him on such a scale, and against the so-called "halo-effect" in which personal prejudice places the examinee either indiscriminately high in all attributes or low in all. This is a system of extremely subjective judgments. Here, very definitely, arises the question whether such ratings are verifiable by all equally competent persons. The nearest approach to objectivity in all this is made by averaging a lot of subjective judgments so that the various raters' prejudices may offset each other. Such a situation would undoubtedly have provoked old Zeno of Elea to inquire, "How can many subjective judgments unite to make one objective judgment?" And, like most of Zeno's questions, posterity must struggle with it.

Personality tests are so difficult that a minimum of scientific value is credited to them. Intelligence tests are considered more objective and hence more scientific inasmuch as the examinee is scored on performance and not on personal estimates. The standard set for any intelligence test, nevertheless, also depends ultimately upon subjective judgments. The standards of Mental Age are based on scores made by *average* groups, but there is no way to average all of humanity. The criteria of what qualities of age, wealth, nationality, environment and so on shall characterize an "average" group for test purposes depend on the personal judgment of researchers, but human material is so heterogeneous that none of these selected groups is at all certain

ever to be duplicated. When the researcher's personal judgment is good, the performance of the selected groups will be closely approximated by subsequent groups, but when it is bad, the standards derived will prove rather useless. Whether they are useful or useless depends largely on the purposes of those who employ the tests, and purpose, again, is a subjective matter. By these fundamentally subjective yardsticks then, the standards for objective tests are set.

Industry eagerly seizes upon objective tests in the hope of a "scientific" solution of personnel problems, especially those of employment. With strictly objective tests of applicants, any competent employment clerk would be able to tell from the applicant's record, without even seeing him or her and so without any personal bias, whether that person is fitted for a certain job. Suppose a business concern requires typists. If the number of words a typist can pound out per minute were a sufficient criterion of efficiency, that would be a matter of objective record. Usually, however, the employer wants to know the typist's adaptability to unexpected demands, the extent of her initiative if given any responsibility, her accuracy in understanding orders, and other qualities which are not a mere matter of enumerated output of words.

Tests have been worked out for these qualities. A page of seemingly routine copying may have unexpected features for the examinee to cope with; a series of "best-answer tests" may be given in which supposed situations requiring initiative are presented and with them are three or more alternative suggestions for action, the best of which is to be checked; or special directions for underscoring letters, drawing lines, filling in blanks and various unusual pencil stunts may be given with time limits such that promptness and accuracy will be revealed in execution of the directions. All these tests can be given to typist applicants in the most objective manner, and can be scored with the coldest impersonality, but how is one to know how high a score in these tests is necessary to meet the employer's standards? The answer is, "By subjective judgments." Employers or supervisors must choose those typists already in their employ who, *in*

their personal opinion, are most dependably adaptable to unexpected demands, who display the proper initiative, and who carry out orders with consistent accuracy. The objective tests will be given to these subjectively chosen typists. If they make an average score of 76 on the tests, then 76 will become the standard for that battery of tests, and future applicants who do not approximate 76 will be rejected. Then to make sure that this objective method is successful in achieving the desired end, the test must be validated. Once more the employers or supervisors must report whether, in their subjective judgments, the typists selected in this fashion have measured up to the standard desired. There are a number of variations on these methods but, in general, the standards are originally set and the tests are ultimately validated by comparison with subjective judgments. Whether subjectively validated objectivity is or is not "scientific" is an interesting question.[3]

In a far more important way than merely in the matter of testing, subjectivity turns out to be the determinant in major laws in the social sciences.[4] The laws of any science, social or natural, pertain to the actions of individuals or groups. The laws of natural science are carried out by particles of matter or units of force acting individually or en masse. The laws of social science, when fulfilled at all, are carried out by people. In the social sciences, it is impossible to exclude all irrelevant factors and to control all relevant factors, and, therefore, the laws of social science are constantly qualified by the proviso, "all other things being equal," i.e. the generalization holds good if other unstated factors remain unchanged. The unstated factor which is *always* present and is least likely to remain unchanged is the subjective attitude of the individuals or groups who must carry out the law. The difference that this subjective intrusion makes

[3] For an interesting view on subjectivity in measurements in physics, see article by Douglas McGregor on "Scientific Measurement and Psychology," Psychological Review, May, 1935.

[4] For my initial clue to this point, I am indebted to an article by Van de Walle, "A Fundamental Difference between the Natural and Social Sciences" in the Journal of Philosophy, September 29, 1932.

between social laws and natural laws can be clearly presented by a contrast of simple laws in the two fields.

A simple, fundamental law in economics is that, all other things being equal, an increased supply leads to a decreased price. This law can be, and sometimes actually is reversed by the subjective vagaries of the very individuals whose behavior the law purports to record. Such a reversal was described in an article in Fortune, 1936. The article exposes the "rackets" of pseudo-wholesalers who throw large supplies of goods on the market in announced "sales." These surplus goods appear in special display rooms and are advertized as bargains, although actually the prices asked are higher than the current prices for goods of the same grade in the stores. This dramatic appeal to the subjective whims of bargain-loving shoppers lures buyers who, unwittingly, pay higher prices for the surplus stock. Increased supply in this case has led to increased price. In the long run, prices could not be boosted, nor be kept high by throwing additional stocks on the open market, but that is why the exception is significant. Subjective factors can temporarily upset, divert or delay the working of social laws and there is no way to calculate under what circumstances, how often or how much they will do so. This is decidedly not a case where outside factors intrude, as sometimes happens to upset anticipated workings of natural laws. To repeat the important point already noted, unpredictability is due primarily to the vagaries of the very individuals whose behavior the law purports to record. An objective law in social fields works if and when the subjective factors of those elements, without which the law cannot operate at all, stay in line.

Contrast to this the situation with a natural law, e.g. the simple law that water, under normal pressure, freezes at 32°F. The natural scientist makes sure that all the elements and factors involved in the working of the law, namely the two gases, the proper pressure and temperature, are present and all else is excluded, or it is measured and allowance is made for it. From that point on, the natural scientist would be dumfounded if the workings of the law were diverted or delayed by the wilful action

of those very elements and factors which were to fulfill the law. The hydrogen and oxygen, he feels sure, will not embarrass him by a sudden subjective whim that this time, they will freeze at 42°, in the same spirit in which human beings may decide at any time to pay excess prices. The temperature will not succumb to a distorted sense of values and refuse to go below 60°, as a merchant may at any time refuse to go below an exorbitant price. The pressure will not defiantly exert unexpected force, after the conditions of the experiment have been set, as price-fixing human agencies might do. The elements which are to carry out the natural law are dependably persistent in objective characteristics. The elements which are to carry out the social law are subjectively erratic in their departures from any course of anticipated behavior. The social scientist has to win the coöperation of his elements before his laws can be consistently effective.

If the people to whom a social law pertains thoroughly understand the law and if they know that the conditions are right for it to work, then they are quite likely to carry it out. If bargain-hunters understand that an increased supply will lower prices, and if they know that the pseudo-wholesaler increases the supply enough to make the difference felt in the market, they doubtless will shop for the lowest prices and force the whole price level down. Imagine, however, the despair of a natural scientist if he thought that the only way to be sure that one of his laws would be carried out on a given occasion, when he wished to demonstrate it, would be to convince his elements that the conditions were right, and that they ought to carry it out.

Sociology is another social science which affords any number of examples of this same predicament. One which makes a ready illustration is Cooley's "Law of the Looking Glass Self." This law states that people know themselves through the reactions of others. It involves three steps; 1) Others receive impressions of person A; 2) These others formulate separately and collectively a system of opinions around those impressions and act accordingly towards A; 3) A observes their actions towards himself and thereby forms his own knowledge of himself. Such is the law. A can upset it if he subjectively cares nothing for the opinions and

actions of others who notice him. He can also upset it if he prefers to look to the example of people in history or in public positions who never knew him at all, and if, from those examples, he sets up a subjective ideal of himself and lives up to that ideal. To be sure, such a move on his part can, by definition be denied to be "knowledge of self," or it can be affirmed to be a subconscious reflection of the opinions of the very associates whom he deludedly thought that he had ignored. By definition, one can exclude all sorts of facts, and to justify a position, one can argue in all sizes of circles, but, on the whole, the fact seems to be that if A knows the law of the Looking-glass Self, and subjectively sympathizes with it, or if his sympathies are subconsciously in that direction anyway, then the law really will work out empirically. Otherwise no one knows when exceptions will occur.

All in all, it seems that, in order to be objective in the sense that every person can verify a law by empirical tests with identical results for all, the social scientists must hope to have their laws so subjectively convincing that people who are to carry them out will accept them. On the strength of that subjective acceptance, the law may be objectively seen in actual operation at any given time. Otherwise, although it may more often be fulfilled than not, and thus justify its statement, the fact remains that, on any specific occasion, only the voluntary coöperation of the elements themselves can insure that that will be the time when it will operate. Such are the embarrassments of objectivity in the social sciences.

Questions which arise from this are the significance of subjectively validated objectivity, and of social laws which can be *consistently* attained only through subjective conviction of those who must carry out the laws, and who thus make it their purpose not to run counter to such laws. This reinvokes "purpose" which has, for so long, been banned from scientific investigations as something which is certainly not objectively independent of knowledge and not objectively verifiable by all competent observers. In this connection there is reason to consider the following pointed comments in A. N. Whitehead's "The Function of Reason":

"The conduct of human affairs is entirely dominated by our recognition of foresight determining purpose, and purpose issuing in conduct. . . . We are, of course, reminded that the neglect of overwhelming evidence (of this dominance of purpose) arises from the fact that it lies outside the scope of the methodology of science. . . . The brilliant success of this method is admitted. But you cannot limit a problem by reason of a method of attack. . . . Many a scientist has patiently designed experiments for the *purpose* of substantiating his belief that animal operations are motivated by no purposes. He has perhaps spent his spare time in writing articles to prove that human beings are as other animals so that 'purpose' is a category irrelevant for the explanation of their bodily activities, his own included. Scientists animated by the purpose of proving that they are purposeless constitute an interesting subject for study."[5]

With purpose frankly reinvoked there may be tremendous significance in objective methods subjectively validated, and in objective laws so subjectively convincing that their fulfillment can be insured.

In summary, a tendency, currently most conspicuous in the Social Sciences, to make objectivity per se the dominant criterion of scientific methods leads either directly back to classical mechanistic science and revives the old problems of dualism or else leads on to a new method paradoxically based on subjective purposes. Right now the blindness of faith in the intrinsic merits of objectivity obscures these consequences. When objectivity means primarily the mutual independence of that which is known and the person who knows, the consequence is a strictly materialistic, mechanistic emphasis. Those who follow this path either flatly deny the reality or importance of anything which cannot be reduced to mechanistic terms, or else admit that, although there are mechanistic phenomena in more fields than anyone formerly suspected, there still is an independent someone or something to know those mechanisms and therefore the old problems of dualism are still with us, and still call for a form of knowledge to supplement science. To paraphrase Aquinas'

[5] A. N. Whitehead, "The Function of Reason," pp. 8 and 12. Parentheses mine.

old standard, any supplementary form of knowledge must not be contra-scientific but may be supra-scientific.

When objectivity primarily means common verifiability by all equally competent observers, and when homogeneity of the material is not present as a guarantee that the characteristics observed in the phenomena will persist, the path is along the line of subjectively validated objectivity, with purpose the inevitable guide. The Social Scientists are meticulously charting this path so other observers may follow it with precision and profit, but all along they scrupulously camouflage it with traditional scientific labels. At every new turn a sign-post marks it as the Highway of Objectivity in order to keep up the confidence that it is Scientific. Subjectivity and Individual Purpose, nevertheless, determine each turn it takes, however objectively it can be followed once the turns have been established and made clear for those who may subsequently travel the road. The time has come to recognize this, and to invite rather than to suppress criticisms on the basis of the following questions: 1) Is this development scientific in anything but a Pickwickian sense, or is it at best only pseudo-scientific and therefore unworthy of full confidence? 2) Is it contra-scientific? 3) Is it, in a proper sense, supra-scientific? Is it a new and better way to meet the problems of dualism in that it is a way beyond the limitations of mechanistic science which does not, in surpassing those limits, sacrifice the claims to confidence which traditional science has so completely monopolized for nearly three centuries?

Department of Philosophy,
Amerikan Koleji,
Instanbul, Turkey.

DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

MODAL LOGIC—A REVISION

Sir:

Since some of my articles on modal logic have had the good fortune to appear in your journal, may I ask the indulgence of a little space for a brief comment?

For a number of years, in the attempt to develop a general logic which would preserve all modal distinctions, I have employed certain expansion formulas to take care of the case, in which no quadrant of the propositional universe vanishes, or again, the case in which the single quadrant $p'q'$ vanishes. The attempt to do this led to a method of exhausting possibilities, which was termed the method of "quadrant analysis."

Quite recently I had observed that one of the expansions of a function of the second order failed to yield an equality, and it seemed to follow from this fact that the expansions of the negative functions were "too long," that the expansions of the affirmative functions were "too short." The repeated attempt, through a very long analysis, to find a term to add to the series, always led to the conclusion, that the correct formula was the very simple one,

$$|\,pq\,| = |\,p\,| \cdot |\,q\,|$$

"p and q are possible is equivalent to p is possible and q is possible."

One of my students, Mr. C. W. Churchman, had suggested this as an alternative to my expansion formulas, but it appeared at that time only as an interesting curiosity. As the result of my analysis, I am now pretty well convinced that it is the *only* possibility. It fails (that is, it does not necessarily hold) for the cases, $pq = o$, $pq' = o$, $p'q = o$, and holds in all other cases. The time and labor spent seem like a high

price to pay for the finding of an assumption so obvious, until one considers this:

While $|pq| = |p| \cdot |q|$ is almost the first suggestion that comes to one's mind, when one first faces the problem, it is also the first suggestion that one would reject (thus, for $q = p'$ it fails: $|pp'| = |p| \cdot |p'|$). But unless the method of "quadrant analysis" was there to save it, I doubt if any one would have thought of inventing it for that purpose.

I may add that the adoption of this formula changes almost nothing in the structure of the system of modal logic itself.

The general equations, for modals of any order and in any number of variables, turn out to be of the simplest form imaginable, viz.

$$Mp.q \cdot \cdot \cdot = Mp.Mq \cdot \cdot \cdot$$

$$Np.q \cdot \cdot \cdot = Np + Nq + \cdot \cdot \cdot$$

where M and N are any modal operators, the first containing an even number of symbols of negation, the second an odd number.

These contain the expansions of sums as well as products, for the sum instead of the product results when the symbol of negation is the first part of the modal operator. Thus, as special cases we have:

$$(pq)' = p' + q'$$

$$(p + q) \angle o = (p \angle o)\,(q \angle o)$$

Another of my students, P. C. Rosenbloom, has developed a systematic method for finding substitutions which invalidate them within the specified regions where they fail.

H. B. Smith.

University of Penna.

REVIEWS AND NOTES

THE ANATOMY OF SCIENCE

The Logical Structure of Science. By A. Cornelius Benjamin. Kegan Paul, Trench, Trubner & Co. 1936. 10/6. Psyche Monographs No. 9.

Professor Benjamin has written here a most "modern" book. He fears labels. If he has chosen to call his position "critical positivism," it is only because he is so critical of positivism and so positivistic about his other criticisms. He sees much of what is wrong in all positions. He even suspects his own.

The things Benjamin is sure of are: (1) All science and philosophy must start with the given, (2) the range of the given is not definite, (3) there are things which are clearly given and obvious, (4) there are things which are obscurely given and conjectural.

"It is the task of science and philosophy to symbolize this *total* realm of the given, through the discovery of relations connecting the clearly given with the obscurely given. In terms of such relations we may *derive* the obscurely given from the clearly given and *explain* the clearly given by the obscurely given. I have tried to meet this difficulty by insisting that the obscure is part of the given. Crudely stated, the obscure is hinted at in the obvious."

Fourteen chapters elaborate in one way or another, the main issues: (1) the place of science, (2) the structure of science. By "place" Benjamin means "place amongst human activities" and the "classification of the sciences." His talisman is the distinction (p. 27) that science, philosophy and history designate three kinds of approach to the intellectual grasp of reality, reflecting three aspects of nature (temporal succession, cross sections and enduring features, or becoming; temporal being; and eternal being). A map of the sciences according to these principles is given compactly. The discussion culminates in assigning to philosophy of science two parts: the study of the methods of the sciences and the study of the categories explicit in the sciences.

"The interrelation of these two parts is obvious. The philosopher of science attempts to determine the meaning of the basic concepts of the science in question with distinct reference to the method which has been employed in the determination of those concepts. He will attempt to determine how the concepts are derived from experience, how their contents are determined by their manner of derivation, and how they are often redefined in order to accomplish a higher degree of integration in the conceptual system of which they are parts. Thus the philosopher of science must analyze the world to determine its features, and science to determine its methods. And he must ascertain whether the methods are adapted to the subject-matter and to what extent the knowledge of the subject-matter is a reflection of the methods. The philosophy of any one science, e.g. the philosophy of mathematics, will consist of these two parts: a logic of mathematics and a metaphysics of mathematics. So also there will be a logic of biology and a metaphysics of biology, a logic of ethics and a metaphysics of ethics, and so on."

How reluctant a positivist Benjamin is can be seen in his treatment of the structure of science. He spends some time in discussing the *possibility* of a logic of science. He is under no illusion. It cannot be demonstrated. What he calls the phenomenological analysis of science is a description of science as:

"an awareness of something which is numerically and qualitatively other than the awareness, and which is related to the awareness in a way which we describe by saying that the awareness is *of* it. This content is a complex consisting of symbolic elements and non-symbolic elements. The symbolic elements are of two kinds: those referring to the non-symbolic elements and those referring to the relations between the other symbolic elements and the non-symbolic elements. If either the awareness or one of the types of content is lacking, there is no science—at least there is no *adequate* science; and if inadequate science is not science, then there is no science."

Further, Benjamin feels the knowledge must be justified (by a symbol) as to its applicability. More obscurely given features of science (as explanation) are given by the requirements that (a) symbols be such as can be applied and (b) objects be such as can be known.

The rest of the book is development of these themes: Nature—occurrents and complexes; awareness; operations; meaning, correlational symbols, constructs and hypothesis; the development of knowledge; models; description; explanation; quantitative method. The concluding sentence is insidious: "It would be surprising, would it not, if science could make the world other than it is?"

Now, can we tell Benjamin where he stands more clearly than he has indicated himself? I should say rather not. It were better to ask him some questions (after reading his worth-while book) thus: (1) Are you a realist? (2) Are you a behaviorist? (3) Why don't you eliminate the manner of metaphysics? Can't you? (4) Why sidestep materialism?

Science deals with matter, does it not? Would you open the gates wide to an idealistic twist to your distinctions, as you seem to quite often? Is your use of terms like space and time, scientific, positivistic, operational? or metaphysical (in the bad sense)? (5) Have you kept to a single criterion of the meaning of *clearly* and *obscurely* or are not some things both clear and obscure in definiteness or certainty depending on context of interpretation (page 84)? Why speak of the "realm" of the clearly given? (page 87). (6) How have you derived *creation* from *discovery* of the given (page 138)? (7) How can awareness of structural relations lead to "*meaningful*" anticipations of experience (page 196), the word meaningful being stressed, I suppose, to mean more than structure? (8) Have you really succeeded in separating quality from quantity (page 326)?

Benjamin has spoken of so many things. Science and its philosophy are such rapidly growing monsters. There are numerous other analyses that can be made along quite different lines. One can only wish that more men would have the courage to press alternative individual analyses. Their work and Benjamin's would then be the subject matter of a general science of the philosophies of science.

WILLIAM MARIAS MALISOFF.

PHILOSOPHICAL HISTORY

Histoire de la Pensée Scientifique, par F. Enriques et G. de Santillana. 3 fasciscules (no. 384–385–386). Actualités scientifiques et industrielles. Hermann & Cie. Paris, 1936. V. I. 76 pp; V. II, 62 pp; V. III, 45 pp. Prices: 12, 10, 8 francs, respectively.

These three serial manuals deal in six chapters with Greek scientific thought over the period of the few hundred years which includes the Ionian "physiologers" or naturalists, the Pythagoreans, the Eleatics, Empedocles, Anaxagoras, and the atomists. The authors published their first account in Italian: *Storia del Pensiero Scientifico*, four years prior to the present French series. They show themselves well at home in the cultural context of Greek thought. Enriques is well known for his mathematical, logical, philological, and historical knowledge which makes him preeminently equipped for the philosophical history of the sciences, as his *Historic Development of Logic* shows.

Philosophical history I take to be history with the usually hidden assumptions of the historian made explicit, and thus made capable of evaluation by critical comparisons with alternative assumptions and

their consequences. The present authors reject both the factual, philological method of positivistic historians like Ueberweg and the a priori predilections of idealists like Zeller. They deliberately adopt the hypothetico-deductive method of Enriques, based on a critical realist's view of hypotheses as continually subject to modification by both thought and experience. The aim which this method envisages in the present work is "an organic vision of the history of Hellenic thought by taking the problem of matter as a guiding thread." (I, 20). Texts and bibliographies are referred to only as controls for whatever interpretations the authors make concerning temporal sequence and logical continuity in the scientific doctrines of the Greek philosophers studied. Thus, monotonous expositions of insignificant details and minor texts as well as fanciful speculations of what science might have been, are both avoided for the sake of a more pertinent synoptic view of the major doctrines already actually incorporated in the progress of science.

The following three theses concerning the period covered are more fully elaborated by the authors than by Enriques in his former writings:

(1) The mathematical importance of the Eleatic critique of motion is stressed in support of Tannery's and Zeuthen's penetrating discussions of Zeno's paradoxes as contributions to the theory of infinite series and infinitesimals.

(2) The historical influence of the Democritean school on the mathematical and philosophical thought of Plato, Aristotle, Galileo, etc. is reiterated in line with Enriques' favorite thesis about the Abderite.

(3) Perhaps due to the collaboration of Mr. Santillana, there is more said about the cultural (social, artistic, religious) background as providing significant factors in tracing the development of Greek thought from place to place.

Following the selective method of the authors, I shall confine my comments to each of these three points:

(1) Notwithstanding the authority of Tannery, Zeuthen, and Enriques, there are good reasons for doubting the importance of the specific mathematical contributions of the Eleatics in the manner claimed by these mathematical minded historians. There is no evidence of any attempts on the part of Zeno or any other Eleatic to perform the summation of any of the infinite geometric series in the paradoxes. A mathematical mind would have surely suggested that the two series in the first and second paradoxes $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \ldots$ and $1 + \frac{1}{10} + \frac{1}{100} + \ldots$ are merely instances of the same infinite geometric progression which has a finite sum $\frac{n}{2}(n + 1)$ (where $n < 1$). Enriques and Santillana have

to formulate and sum the latter series themselves $1 + n + n^2 + \ldots$ (II, 50) in their desire to credit Zeno with the knowledge of the sum or integration of such an infinite series, adding "about that time, the gometers, if not Zeno himself, discovered *the sum of the infinite geometric progression*" (*ibid*, italics not mine). Why is there not the slightest indication in any of the Eleatics of such a summation? The omission indicates either ignorance on their part of the contemporary work of the mathematicians or a refusal to consider such a mathematical operation as relevant to the resolution of their paradoxes of the unintelligibility of motion. Since Enriques denies both these consequences he should also deny that Zeno omitted such a summation. This he can do only by appealing to inferences based on the temporal and geographical proximity of the Eleatics to the Pythagoreans, but he cannot offer any evidence for such a summation from any part of the extant texts.

There is, however, a more general logical aspect of the question which would permit agreement with the authors that the *operation* of infinite divisibility is common to the paradoxes of Zeno and the subsequent mathematical method of exhaustions used by Eudoxus and Archimedes. It must be granted also, on the logical side again, that Zeno's paradoxes are brilliant examples of the *reductio ad absurdum* type of demonstration which is so powerful in proving the converse of a mathematical theorem (II, 51).

(2) There is much more evidence in favor of the authors' thesis that Democritus actually contributed to the mathematical analysis of infinite series and infinitesimals. Democritus stated the problem in a manner more specifically pertinent to the mathematical problem than Zeno. The authors could have used Plutarch's statement of Democritus' dilemma (*De comm. not. adv. Stoicos*, xxxix, 3):

> "If a cone is cut by a plane parallel to the base, what must we think of the surfaces forming the sections? Are they equal or unequal? For if they are unequal, they will make the cone irregular with many indentations, like steps; but if they are equal, the sections will be equal, and the cone will appear to have the property of the cylinder and to be made of equal, not unequal circles, which is very absurd."

Heiberg, commenting upon the above dilemma of Democritus (*Gesch. der Math. u. Naturwiss. im Altertum*, 13) likewise says that it alone "allows us to suppose that his atomic theory had brought him near the infinitesimal method which Eudoxus founded and on which Archimedes built further."

Since Eudoxus and Archimedes are much later than Zeno, it seems, again, misleading to attribute to the dialectical arguments of the Eleatics

rather than to the atomistic analysis of Democritus credit for the mathematical analysis of infinitesimals and their integration. For it was Democritus who actually found the volumes of solids like the pyramid and applied the notion of infinitesimals to physical analysis. To attribute more than the origination of the problem in Greek scientific thought to the Eleatics is as dubious as to attribute the first formulation of relativity theory to say Bradley's dialectical critique of space, time and motion. Denial of such a direct dependence of scientific results upon metaphysical analysis does not imply, of course, the positivistic denial of any significant metaphysical analysis of the categories and logical structure of scientific thought. The Eleatics inspired or provoked Plato in the *Parmenides* and *Sophist* to some searching analyses of some of these categories.

(3) The fragmentary character of Greek texts offers a serious obstacle to facile generalizations which correlate Greek scientific thought with the social milieu. The authors' brief and rather fascinating account of the historical connection between the commercial life of the Greek colonies and the scientific doctrines discussed in the Ionian, Italian and Athenian schools, is extremely suggestive. Moritz Cantor's own monumental contributions to the history of mathematics were motivated by such problems. What remains to be done in the spirit of Cantor's work will undoubtedly require the collaboration of philologists, historians, scientists, and philosophers, even if the latter act only as gad-flies to sting those clumsy and slow-moving bibliographers, who never question first principles, into critical reflection over the guiding posts of their researches. The work of Enriques and Santillana shows that philosophers may even do more than gad-flies by fertilizing the field of historical scholarship through diligent researches of their own.

PHILIP PAUL WIENER.

College of the City of New York.

EMPIRICAL PROBABILITY

Wahrscheinlichkeit Statistik und Wahrheit. Einführung in die neue Wahrscheinlichkeitslehre und ihre Anwendung. Zweite, neubearbeitete Auflage. Richard von Mises. (Schriften zur Wissenschaftlichen Weltauffassung, Band 3.) Wien, Julius Springer. 1936. viii + 282 pp. RM 16.

Those who are already familiar with the first edition of Professor von Mises' study will welcome this new, enlarged, and partially rewritten

edition. This book is of a type unfortunately far too rare in the field of the philosophy of science: a non-technical but sober and critical examination of fundamental problems.

Professor von Mises views the theory of probability as an empirical science, differing from other empirical sciences by the special subject matter with which it deals, and not by a special kind of logic or methodology. For a scientific treatment of this subject matter it is essential that an empirically applicable definition of probability be available. The only rational definition which allows both empirical application and quantitative treatment by the calculus is one founded upon a properly conceived relative frequency notion. This requires a collective, i.e. a mass or repetitive phenomenon which fulfills two conditions: the relative frequencies of the specific characters of the elements within it must approach definite limits, and the limits must remain unchanged upon any arbitrary selection of part of the elements which involves only their place within the series (principle of irregularity). If these conditions are satisfied, the limits of relative frequency are called the probabilities of the appearance of the characters within the given collective. The task of the calculus of probability thus becomes: given certain probabilities, to derive new probabilities from them. Since any assertion of probability refers to a limit of relative frequency *within an observed or observable lengthy series of occurrences*, it is always an empirical hypothesis; and the calculus, by drawing consequences from such hypothesis, provides an indispensable tool for systematizing and testing them. This function is first exemplified by a treatment of de Méré's problem and then profusely illustrated in applications of probability theory in social, biological, and physical statistics—a procedure which throws much light upon these fields.

Professor von Mises recognizes and insists upon the limitations of the frequency interpretation: probability in this sense can only be spoken of in the presence of a collective. We cannot apply it to single cases, say the death of an individual, no matter how much evidence we possess about him; the expression is meaningless here. (The probability of theories, which remains also an outstanding instance of which the frequency theory has as yet either to offer an explanation or to reject all explanation, is not discussed.) The section "Kritik der Grundlagen" has undergone extensive enlargement. In what has been added the author attempts to evaluate some of the most urgent criticisms of his theory recently advanced. The last section, on physical problems, has also been extended to take care of the new direction given to phys-

ical statistics by quantum mechanics; this discussion is especially illuminating.

It cannot too strongly be said that Professor von Mises, writing in a form primarily determined for non-mathematicians, dispensing almost completely with formulae, has succeeded in accomplishing a most important task of scientific philosophy, the clarification of basic concepts.

ALBERT HOFSTADTER.

A MEMORY CONTINUUM?

The Psychology of Belief. By Dr. Helge Lundholm. Duke University Press, 1936. Pp. 245.

In a previous monograph, *Conation and Our Conscious Life*, Dr. Lundholm formulated a comprehensive psychological doctrine based on three assumptions: (1) that conation or purposive striving is the basis of all mental life; (2) that in the present state of physical knowledge it is futile to try to explain mental life by physical law; (3) that the animistic interpretation of the behavior of all living things leads to a better understanding of animal conduct than any other. Thus the belief in conscious guidance of behavior on all levels of life is an application of the dualistic-interactionistic theory.

In the present volume the author, while building upon the work of James Ward, G. F. Stout, S. Alexander and William McDougall, adds some novel ingredients in the form of the concepts of "primordial curiosity," "primordial deference," the "memory-continuum," etc. This instinct or urge psychology is then applied to *Beliefs*. By suitable emendations of McDougall's thesis concerning "Belief as a Derived Emotion" Lundholm shows that the sources of belief are two, emotional and rational, the former being rooted in the apprehension of things in their ego-relations and the rational (reality) function of the mind being a grasping of the relations of objects to each other and to the universe at large. The emotional basis prevails in children in their fairy-land beliefs, which are normally corrected in adults by the consideration of things in their universal connections, except when, as in manic-depressive psychoses and schizophrenia, the emotional principle of belief engenderment leads to delusion-formations. Thus Dr. Lundholm presents a theory which purports to comprehend the logical, the normal psychological and the pathological under common explanatory principles.

To a philosopher the most interesting concept in this system is likely

to be that of the memory-continuum, a "metaphysical" concept covering all the mind-units ranging from the hypothetical cell-mind at the bottom to the hypothetical deity at the top. Since, as Dr. Lundholm believes, psychical research indicates that memories survive bodily death, and since evolution is purposive (as indicated by the rôle of instincts or racial memories in evolution), the author concludes that the individual memory-continuum is a part of a wider field, eventually an infinite memory-continuum akin to the transmittant universal mind of William James or the super-human monads of Leibniz. While Dr. Lundholm does not state this in the present book, he has elsewhere indicated that he believes that he has here a theory which may explain the results of his colleague, Dr. J. B. Rhine, in the field of *extra-sensory perception*. Of course thinkers with positivistic-behavioristic leanings will condemn the whole business, but perhaps the work and influence of Dr. McDougall and his colleagues at Duke University may yet accomplish what McDougall's influence at Harvard could not. However that may be, the value of Dr. Lundholm's work must be judged on its own merits, and from this point of view it is an important contribution to the study of the human mind.

Oliver L. Reiser.

University of Pittsburgh.